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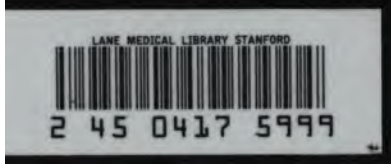
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CITY OF BOSTON.

REPORT

OF

CITY PHYSICIAN.

1867.

In Board of Aldermen, March 11, 1867.

Laid on the table, and eight hundred copies ordered to be printed.

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CITY OF BOSTON.

CITY PHYSICIAN'S OFFICE.
JANUARY 1, 1867.

TO THE HONORABLE THE CITY COUNCIL OF THE CITY OF BOSTON:

At the close of another year, and in accordance with usual custom, I have the honor respectfully to present the following as my Annual Report for the twelvemonth ending December 31, 1866:

Six hundred and forty visits have been made to persons confined in the jail. There has been no prevalent sickness the past year, and, with exception of two cases in one of which the disease was contracted before admission, there has been no small-pox. In the cases referred to, immediate removal, thorough vaccination of the other inmates, and such other precautionary measures as were deemed necessary, prevented any further spread of the infection. I am happy in being able to add, that, since my last Annual Report, the evil therein alluded to, relating to the need of better accommodation for the sick, and more ample bathing facilities, has been remedied. The changes made under the direction of the Committee on the Jail leave nothing to desire in these respects.

All necessary attendance has been rendered to those who, during temporary confinement in the City Prison, better known as the Tombs, have needed the services of a physician.

The Temporary Home in Charles Street has been visited, whenever medical aid has been required. Four (4) deaths have occurred here during the year, — these being foundlings, who died in a few hours after being received.

26694

By request of the Chief of the Police, thirty-five (35) appointed men have been examined, and certificates given them of their physical ability to serve on the force.

Frequent examinations of localities, reported as being in a state of nuisance, have been made during the year, and their condition reported to the Committee on Internal Health.

By request of the City Registrar, three (3) persons, dying with no physician in attendance, have been examined, and the proper certificates returned to the office of the Registrar.

Six hundred and thirty-six (636) soldiers have been examined, and certificates of disability given them, under the direction of the Relief Committee.

During the past year, the city has been remarkably free from small-pox. By the records in the office of the City Registrar, forty-nine (49) deaths have taken place from this disease, against one hundred and fourteen (114) in 1865, and one hundred and thirteen (113) in 1864.

This number is comparatively small, but it is a matter for grave consideration whether the advantages, in a humanitarian point of view, which were set forth in a memorial presented to the Legislature of this State by the late "Dr. John C. Warren and thirteen other physicians of Boston," praying for the repeal of the provisions of the statutes then in existence relating to removal of persons sick with small-pox, and which resulted in their repeal, have not been largely outnumbered by the great increase of deaths from this cause since that time, — an increase which is altogether disproportioned to the increase in our population. By reference to City Document, No. 30 (April), 1856, which is a "Memorial in relation to Small-pox presented to the City Government of that year, by the late Lemuel Shattuck, it appears that in the twenty-six (26) years previous to the repeal of the statutes alluded to, from 1811 to 1836 inclusive, there were thirty-eight (38) deaths from small-pox in a total of thirty thousand three hundred and thirteen (30,313) deaths from

the exception of nine (9) years — 1837, 1838, 1844, 1845, 1847, 1848, 1849, 1852, 1853, — is larger than *the whole number for the whole of the first period already alluded to.*

From whatever stand-point it may be viewed, — whether the subject be considered with reference simply to mitigating the suffering consequent upon the presence of this most loathsome of all diseases, or as a matter of pecuniary cost in diminishing the income from labor, — it assumes an importance which compels attention, and makes it worthy the most serious consideration of the Board of Health, as a measure of political economy.

The City Government, to be sure, has provided a way in which all this *can* be remedied, by affording, to all who will avail themselves of it, gratuitous vaccination. But the number — about three thousand (3,000), year by year — of unvaccinated persons who are always to be found in the city shows that there is the greatest indifference on the part of the community to avail themselves of this boon. No one doubts, that, if a complete and thorough vaccination of the inhabitants of Boston could be enforced, small-pox would be suppressed, and our citizens be spared such visitations as, within the past five years, have twice overrun the whole city, leaving not a street, and hardly a house, without its foul presence. I respectfully suggest, for your consideration, the subject of compulsory vaccination and removal of those persons who fall sick with small-pox, with a view of obtaining such changes in the existing laws as will more effectually protect the community.

Since my last Annual Report, Asiatic cholera has once more effected a landing on our shores, and made its appearance amongst us. Although its victims have not been many, comparatively speaking, it has taken from this community those who could ill be spared, and whose loss is a public misfortune. In the latter part of the year 1865, as will be remembered, reliable information from abroad notified us of the approach of cholera, by its usual route from the East. Forewarned by this, a plan

for a thorough sanitary survey of the city was adopted by the Board of Health, and in the early part of the season was put into operation. The city was divided into sanitary districts, in each of which a sufficient number of inspectors were appointed, who, under the direction of the health wardens of that district, made a thorough and careful house-to-house visitation, and reported the results to the central office. By this course, a double object was achieved. Not only were the localities ascertained where the especial attention of the health department were needed, but the community were awakened to a sense of their own responsibility in the matter, and thus most efficiently aided the city authorities in their work.

By the returns made to the Registrar, there were eleven (11) deaths by Asiatic cholera, and forty-six (46) by cholera morbus. By the answers to a circular letter, addressed to the physicians of Boston and vicinity, by a committee of the Suffolk District Medical Society, of which the undersigned was a member, this relative number is considerably changed. This discrepancy is only another proof of the great difficulty, in a season when both are prevailing, side by side, of absolutely separating the two diseases, and discriminating between them. Of a few, reported to the Registrar, no details could be obtained, and the following reports must be taken as the best history of the disease, as it appeared here the past season, that could be compiled.

No. 1. July 18.

Calvin L. Farnham. Is a soldier, and has been stationed at Harts Island, in New York Harbor. Had a painless diarrhœa for some days previous to his leaving for home on a furlough. Was taken with vomiting, and increased purging on board the boat, which he attributed to sea-sickness. On his arrival in this city in the morning, was so ill as to be unable to go any farther; cold, severe cramps; skin blue, — in short, he had all the symptoms of collapse. He was taken to the house of a comrade, where he received such treatment as brought on very considerable reaction.

In the course of the day, he was removed to the Soldiers' Rest, in Summer Street, then under the care of the late Dr. William E. Townsend, who spared no efforts to relieve him, but without avail. The reaction subsided, collapse came on, and he died in the afternoon. He reported that, at the barracks where he had been stationed, many soldiers had died daily of a disease similar to what was affecting him, and that it was there called Asiatic cholera.

No. 2. JULY 21.

H. C., Age, about fifty, harness-maker, works in Boston, and resides at No. 7 Cottage Street, East Boston, locality on a side hill, well drained, ventilated; was seized on the night of the 21st of July, with severe vomiting, purging and cramps, was alone and could not make himself heard so as to obtain assistance till near morning. He had previous diarrhœa. I found him with severe cramps in arms, hands, legs and feet; vomiting still continued, and also the discharges from the bowels which looked like rice-water, and showed no trace of bile; bile was also absent from the evacuations of the stomach. Pulse quite feeble, but easily felt; voice feeble, urine entirely suppressed, and continued so two days; no corrugations or blueness of surface.

Treatment consisted in giving one grain solid opium every hour, till vomiting, purging and cramps ceased; and whiskey, which he preferred to brandy, *ad libitum*. Recovered. He had not been exposed,so far as I can ascertain, to any of the conditions mentioned in your circular. No other case occurred in the house or vicinity. Reported by DR. P. M. CRANE.

No. 3. JULY 23.

Mrs. A. F., No. 96 Meridian Street. House situated on marsh land, but well drained and ventilated. Mrs. F. is about fifty years old, is subject to bilious derangements, was seized with purging and vomiting two days before I saw her, which was on the 23d of July. I found her with severe cramps, vomiting and purging; pulse very feeble; hands and feet cold; slight approximation to blueness of surface; voice feeble; discharges from bowels, without

bile and resembling rice-water; skin on the hands very much corrugated, like the hands of a washer-woman; urine suppressed.

Treatment consisted in giving one grain solid opium every hour, brandy freely, mustard to feet, legs and epigastrium. Recovered. Had not been exposed to any of the conditions mentioned in your circular, so far as I am able to ascertain.

No other similar case occurred in the vicinity.

Reported by DR. P. M. CRANE.

No. 4. JULY 23.

The undersigned reports ONE CASE OF CHOLERA, at No. 16 Bridge Street. July 23, 7, P. M., Mrs. W., thirty years old, Irish, mother of three children. Reported to have been sick for twenty-four hours, the last twelve, having had frequent watery discharges, and vomiting; the last four hours, cramps in the calves of the legs; no urine since last night.

Now vomiting and purging, cramps, loss of voice, pulse small (130), skin dry, but cool, pinched and bluish, eyes sunken and bloodshot.

24th, at 8, A. M., vomiting and cramps; ceased after midnight; has had two or three rice-water discharges; one urine; skin and pulse the same.

7, P. M., no dejections or vomiting; ʒ v. or vj. of urine passed. Has slept half an hour at several intervals, and expresses herself as better.

24th, 8, A.M., more color, better pulse (100), and has slept several hours; more urine.

25th, 9, A.M., one small dejection, with some odor and color; convalescing; with appetite, but feels very weak.

TREATMENT. — Hot rice-water, well peppered, and strong coffee, — ʒj. of each, alternating at intervals of two hours; sulphuric acid, gtt. v. in ʒ i. of water every hour, with ʒ ss. of beef tea. No other food or drinks permitted. Dry warmth to abdomen and legs.

LOCALITY, ETC. — Tide-water sometimes in cellar; chickens in the yard, which was small and dirty, — a drain from a sink, used by families up stairs, discharging into it; house ill ventilated and over-crowded.

Reported by DR. H. G. CLARK.

No. 5. Aug. 16.

Perhaps an hour or two before day, I was called to see Mrs. R. Age, about fifty-five years. She had not been feeling as well as usual for several days previous, but still did not call herself sick. She had retired early, on account of an uncomfortable state of the bowels, attended by slight nausea and diarrhœa. About 11 o'clock, was seized with vomiting and purging, the uncomfortable feeling changing into actual pain. I found my patient with a cold clammy surface; lips, face and hands, livid; small and feeble, but slightly accelerated pulse; severe pains in abdomen and cramps of lower extremities; finger-nails blue; hands and feet of an icy coldness, — in a word, she was in the first stage of collapse. The vomiting and alvine discharges had been frequent and distressing since the attack, and she was again calling for the chamber. I stepped into an adjoining apartment, but could hear, as distinctly as if it had been forced from a pump, the sound of the fluid as it was discharged into the vessel. On examination I found the evacuation large and presenting all the characteristics of cholera. There was no time to be lost. Every effort was at once made to check the discharges, alleviate the pain, arouse the capillary circulation, and restore warmth to the surface and extremities. I at once administered a full dose of morphine from my pocket-case, to save time; ordered the patient well wrapped up in flannel blankets; artificial heat by means of bottles of hot water, etc.; diligent and persevering frictions to extremities; sinapisms to feet, epigastrium and abdomen. Fortunately, the morphine was retained, and I sent for the following mixture:

℞. Tinct. Opii, ʒ ii. Syr. Zingiber ʒ i.
Tr. Camph. Tr. Capsic. āā ʒ i.
M.

S. One teaspoonful every hour.

At my second visit, an hour or two later, there had been but one small dejection from bowels, but of the same peculiar rice-water appearance; no vomiting; pulse slightly improved; surface warmer; continue the remedies; small bits of ice and toast-water, to quench thirst, — which is less urgent than usually observed where

the loss of fluids has been so enormous. Saw her again about noon. Everything looks promising. Pulse better; surface warm and assuming its natural color; stomach and bowels quiet; pains gone; cramps, none; the opiate mixture to be administered *pro re nata* in slightly diminished doses; has voided no urine, but bladder not distended; add Mucil. Gm. Acac. and Spts. Nit. Dulc. to treatment.

Evening. — Find my patient comfortable every way, but has passed no urine; continue treatment with addition of beef tea in small quantities.

Friday Morning, 17th. — Mrs. R. has had a quiet night; no alvine discharges, but the kidneys had resumed their functions, and a small quantity of urine had been discharged; pulse still improving; action of heart and lungs normal; continue mucilages and diuretics, with occasionally, as may be required, half a teaspoonful of the opiate stimulant; broths and jellies.

Evening, 17th. — Continues to improve; pulse fuller and stronger; kidneys more active; no nausea, bowels quiet; anodyne at bed-time; no medicine in night unless a change of symptoms should occur.

Saturday, 18th. — Has had a good night; countenance natural; pulse as last eve; convalescence fairly established; no discharge from bowels for two days, and so direct the following:

℞. Syr. Rhei. Arom. ℥ i.
Extr. Sennæ Fl. ℥ ss.
M.

S. ℥ ss. of the mixture every four hours, until the bowels are moved, when an opiate is to be administered. From this time there was an uninterrupted convalescence, and the patient left her chamber in about one week from the attack. I have, in other years, seen and treated a great many cases of this disease, but have seldom met with one so perfectly under the control of remedies, where the symptoms were so threatening at my first visit. But this is the first and only clearly marked case of cholera that I have seen during the present season. There was the most unrestricted intercourse between my patient and her family and friends. No precautions whatever were taken to prevent the spreading of

contagion, if any there was; and no evil consequences followed this course of proceeding, not even so much as a mild case of diarrhœa among the family and attendants. Mrs. R. herself had been in no way exposed to cholera, nor had she been guilty of any indiscretions in diet or otherwise, and the location in which she resides is now perfectly healthy.

But it might be mentioned, as a remarkable coincidence, rather than in light of cause and effect, that in the epidemic of 1849, there were seven deaths from cholera in two adjoining houses on Border Street, one of which is the present residence of my patient. In 1849, however, the street had no drainage, and the cellars of these two houses had two feet of standing water in them, as offensive as any bilge-water confined in the hold of the most filthy old ship. But, the year following, a common sewer was constructed in this street, the houses thoroughly cleansed and refitted, and have ever since been in an unexceptionable sanitary condition.

Reported by DR. DANIEL V. FOLTS.

No. 6. AUG. 19.

John Butler, 61 years, laborer, No. 14 Plympton Place. Was called in haste on Sunday morning, Aug. 19, at 1.15 o'clock. On my way to the house, I learned from his son, that his father had been the day previous working with his hand-cart on Central Wharf, that he had eaten a dinner of corned beef and cabbage and drank lager beer, that he had drawn his cart from the wharf to the house, a distance of over two miles (the day being excessively warm), and had arrived at home much fatigued. While on the wharf, he had a slight involuntary discharge in clothes. He retired, feeling well. About midnight, the family were aroused, and found him suffering from violent cramps in legs and arms, and that he had been vomiting and purging. On my arrival, gave ¾ grain Sulp. Morphia on tongue, which was immediately rejected by stomach, together with a pint of fluid containing about a tablespoonful of coffee-colored particles. I then gave Tinct. Opii and Brandy, equal parts, every fifteen minutes. Applied hot water in bottles to legs and feet, and sinapisms,— taking them off when pains ceased, and re-applying. He had at one time slight cramps in

stomach, which were relieved by the same measures. Under this treatment, his pulse returned, feet and hands grew warm, discharges less frequent. Pulse, 112. I left with orders to give a teaspoonful of brandy every half-hour. In three hours, saw him again; learned that he had had only two discharges, amounting to about a pint of fluid, of the same character as before, and no return of cramps. Gave gtt. xxx. Tr. Opii. Ordered a tablespoonful of iced milk every hour. At the suggestion of Dr. Sheldon, who saw him at that time, I gave one of the following powders:

℞ Hyd.-Chlor. Mit. grs. v.
Sulph. Morph. gr. i.
Sacch. Alb. gr. xx.
℔ Fiat Chart. No. iij.,

in afternoon and evening. They produced the first bile colored stools I had seen since attack. He continued in a febrile state for nearly two weeks, not leaving the house for some time after recovery. There had been no cases of choleraic disease in the locality where he lived. The house he lives in is dry,— drainage good. I could not learn that he had been exposed, either upon the wharf or elsewhere. Disinfectants were freely used, and no other cases occurred in the family.

Reported by W. M. Ogden, M. D.

No. 7. Aug. 31.

Have seen and treated one case, M. K. Age, about twenty-eight, machinist, lived at 86 Warren Street. There was no preliminary diarrhœa; he had cramps, and suppression of urine. Rice-water discharges were reported, but not seen; there was also vomiting; he showed no decided blueness, but had a sunken appearance; perhaps skin somewhat shrivelled; pulse very feeble. The treatment consisted of equal parts of Tinct. Cinnam., Tinct. Opii. and Tinct. Catechu, which were prescribed before patient was seen by me; and rubbing with dry mustard. He recovered. There was no known exposure, but he had worked unusually hard. I have heard of no case following exposure to this patient.

Reported by C. Ellis, M. D.

No. 8. SEPT. 2.

MRS. P. W. East Canton St. Age, forty-three years, and mother of eleven children. Was attacked with diarrhœa; some pain in the bowels, and severe nausea at about noon of September 2, 1866. At about nine o'clock, P.M., she considered herself better of the diarrhœa, and did not call for professional aid until five A.M. September 3, I saw her a little before six o'clock, and remained with her about an hour.

Rice-water discharges had been frequent and profuse; a large amount of epithelium was seen floating in the vessel that was last used; did not examine it with the microscope. Cramps very severe in left side, also in legs, but not so severe as they had been during the night previous. Discharges at this time involuntary. Surface very cold and dark and much shrivelled. The patient's mind being very clear, I asked her if she had passed any urine, and she answered No; but the attendants gave a different report, which was that she had been passing water all of the time, and had passed nothing but water. I believe both parties were right; but that the "water" was from the bowels, not from the bladder. Pulse at wrist not perceptible at six, A.M.; at ten, thought I could detect a slight pulsation. The treatment consisted of external heat, frictions and stimulants, beef tea and subcutaneous injections. Opiates had been given during the night before I saw her, but were immediately rejected by the stomach. The first injection was made upon the left side, near the region of the heart, where the cramps were at that time most complained of. The cramps subsided in about ten minutes after the injection of one-fourth of a grain of Sulphate of Morphia. She continued very restless and thirsty until 10 o'clock. The cold, blue, shrivelled and cadaverous appearance continued also. She died at eleven, A.M. I could not produce any effect whatever by treatment, except that the cramp ceased a little earlier on account of the injection. The patient was plethoric, and had, in the absence of a servant, been at work over a hot range in the kitchen for two or three days prior to her attack. She had not been exposed to any other cases of cholera or diarrhœa, nor had she lived in any infected place. She had been an unusually healthy woman,

and a person of active habits. I have known her about eight years, and have delivered her of three strong children during this period. She has never had any sickness during my professional acquaintance with her, except a mammary abcess. I advised disinfecting some of the bedding and burning the rest, which was done. The room was aired and cleansed, and was immediately occupied. No other cases occurred in the house, although the family was large, and had all been in the room during the latter part of her sickness. No case followed exposure to the dead body.

Reported by DR. L. R. SHELDON.

No. 9. SEPT. 3.

On my return from case No. 8, Canton Street, a messenger was waiting for me to go as soon as possible to see Mrs. C. S., age forty four, on Washington Street. I saw her at once, and was quite shocked to find just such a case as I had left on Canton Street. Before I could carefully examine her, she was assisted to get out of bed and into a night chair. She passed at this time at least three quarts of rice-water, liquid, with epithelium floating upon the surface. She was lifted to her bed, and seemed entirely prostrated; vomiting and cramps had nearly ceased. One quarter of a grain of morphine was given in brandy and water immediately. The patient had been attacked (September 3rd), at 4 o'clock, A.M. She told her husband that she felt very sick, and she must go to the water closet. She had four discharges in two hours, previous to this last discharge above mentioned.

There had been no preliminary symptoms whatever, and she lived in a nice modern-built house, the drainage of which was perfect. She had at command all of the usual comforts of life, and was a devoted wife and mother. She had an attack of diarrhœa about six weeks previous, which was quite severe; but yielded readily to treatment. She considered her health better after the attack. Her temperament was nervous-sanguine; weight, 115 lbs. She had not suffered from over-fatigue or excitement. Cramps came on soon after the commencement of the attack.

Blueness and shrivelled skin were very marked, the fingers being

nearer square than round; the dark color extended up the arms, and in fact over the whole surface, which was also cold and clammy. Urine entirely suppressed; pulse at wrist not perceptible. Mind clear to the last. Lost her speech two hours before death, which took place at 3 o'clock, P.M. Treatment: external heat; stimulants; opiates; water acidulated with sulphuric acid and beef tea injections. She had not been exposed to other cases, nor had she visited infected places.

No special precaution was taken in this case after the patients' death, except ordinary washings and cleansings, — the room being continued as a nursery as usual. The under bedding was continued in use, and the body was kept two days or more; the corpse was kissed repeatedly by all of the family and friends. No other case occurred from said exposure.

The sanitary condition of the district adjoining the residence of this patient, of which Fellow's Court forms a part, is, beyond question, extremely bad. Reported by DR. L. R. SHELDON.

No. 10. SEPT. 5.

Miss P., aged twenty-eight years. Residence in a very healthy part of Brookline. She came to visit her brother, in Boston, who resides in Chester Park, and was taken with vomiting of a colorless fluid, on the 5th day of September, the second day after her arrival in the city. I saw her on the morning of the 6th, and found her quite comfortable, with the exception of weakness and the occasional vomiting. Her general appearance was such as to create some apprehension; but she told me that she was subject to turns of vomiting and headache. There had been no preliminary diarrhœa. There *was* vomiting of rice-water material. On the 7th of September, cramps in legs came on, but were not severe. Blueness and shrivelled appearance of the skin were scarcely to be observed at 8, A.M., on the 7th. The urine was suppressed; the surface slightly cool. Pulse feeble; no apparent occasion for alarm at 9 o'clock, A.M. I saw her in the condition described at 9, A.M. She was at this hour in a jocose state, and talked quite freely in that vein. At 11, A.M., I was sent for in haste, and,

going immediately to her bedside, found her dead! The treatment at first was heat to the extremities, sinapisms to the stomach, and the following powder:

℞. Hydrarg. submur. gr. iv.
Morph. sulph. gr. i.
Ft. cht. No. iv.

one to be given with dry sugar, and repeated, if the first was rejected by the stomach. She retained the first about two hours, when it was rejected, and another given. This course was followed during the night of the 6th, each powder being retained longer than the preceding one; yet no color made its appearance in the matter vomited. A remark that she thought she should feel better, if she should have a movement of the bowels, led me to examine the condition of the abdomen, where I found no distension and no tenderness. There was no desire to go to stool. I did not give a cathartic, as she desired, but advised stimulants freely. This was at 9, A.M.; she died at 11, A.M. As soon as she was dead, a very large quantity of the rice-water discharge passed from the bowels, enough to completely saturate her bed. A large amount of epithelium was found in it, as was shown by examination by the microscope. I could not learn that Miss P. had been exposed to any other cases of cholera, or to cases of diarrhœa, or had been guilty of any personal indiscretions. She had not lived in or visited places known to be infected. Every precaution was taken to destroy and to disinfect clothing, bedding, and to purify the room. No other sickness of a severe form followed in this house.

At the same time that case No. 5 was attacked, a Mr. A., aged nineteen, was taken, on the opposite side of the street, with vomiting and purging of rice-water; cramps and blueness of skin, which was also shrivelled; urine suppressed; pulse scarcely perceptible. This patient recovered, after a protracted illness.

Reported by DR. L. R. SHELDON.

No. 11. SEPT. 5.

Mr. C. W. C., age seventy-six years. On Sept. 5th, came directly from State Street, and arrived at his house, near Chester Park, at 2 o'clock, P. M., feeling, as he says, perfectly well for some ten minutes after he reached the house. He then had a sudden call to the water closet, where he had a profuse discharge from the bowels, which caused him to feel very weak; but he got to his bed, which was on the same floor. He very soon vomited a large amount of colorless material, which was followed by extreme prostration. I was at this time called up to see him. I gave him at once two ounces of old French Brandy, with two teaspoonfuls of the Tincture of Ginger.

Being obliged to leave him for half an hour, when I returned I found that he had vomited again, and had had severe cramps in the extremities. The surface was cold, shrivelled, and very livid; discharges involuntary and colorless, but had a slight fecal odor. Another large portion of brandy and ginger was given immediately, and was retained by the stomach. External heat and frictions were used thoroughly. Reaction took place in about one hour.

Reported by DR. L. R. SHELDON.

No. 13. SEPT. 7.

M. F. German, laborer, aged forty, of good constitution and health; resides No. 3 Rockingham Court, a locality on marsh land, but well drained, and in a very exposed situation. Was taken with great looseness of the bowels, without pain, on the 7th of September. Says the discharges came away from him like water out of a barrel; discharges continued in the evening, when I saw him, attended with severe cramps and vomiting. Rice-water appearance, no bile, no suppression of urine. Pulse not much reduced, great mental depression, thinks he shall die; no blueness of surface, or corrugation of skin. Treatment: one grain solid Opium every hour, Whiskey freely, Mustard of surface. Recovered. No known exposure to the conditions mentioned in your circular. No other similar case occurred in this vicinity.

Reported by DR. P. M. CRANE.

No. 14. Sept. 7.

At eight o'clock, on the evening of Sept. 7th, 1866, I was called to attend James W. Goutier, aged 23 years, a sailor, who had returned a week previously from Philadelphia.

On my arrival at his residence, No. 122 Bremen Street, I found him suffering from severe cramps in the stomach and bowels, extending to both upper and lower extremities, the pain and cramps being so severe that it required two or three persons to hold him in bed during the paroxysms. He had severe vomiting and purging, the evacuations from the bowels being of the peculiar character known as rice-water discharges, containing not the slightest trace of bile. The pulse was almost imperceptible; the skin cold and moist, of a peculiar bluish cast, and shrivelled, as if parboiled; eyes sunken; features contracted; tongue cold; and complete suppression of urine.

During the interval between the paroxysms, he remained in a semi-conscious condition, from which he was aroused with difficulty, appearing to be wholly indifferent with regard to his condition or what was transpiring around him. In the course of five or six hours after I first saw him, the vomiting and purging ceased, when he fell into a state of perfect unconsciousness, which continued until his death, which occurred at two o'clock, P.M., on the following day.

On inquiry, I learned that, about two days after his arrival from Philadelphia, he was attacked with diarrhœa of a very mild character, for which he received no treatment, refusing to consult a physician when requested to by his friends, stating that it was unnecessary, on account of the mild nature of the disease. He continued in this condition until the morning of Sept. 7th, when he complained of cramps in the toes, which gradually extended to the legs, bowels and upper extremities, accompanied with severe vomiting and purging. The disease continued to increase in intensity until evening, when, as already stated, I was summoned to see him.

With regard to the treatment, I may briefly state, that I prescribed such articles as Chloroform, Opium, Capsicum, Camphor,

beef tea and Brandy, as the exigencies of the case seemed to demand, with powerful rubefacients and friction externally, and all without apparent effect.

I am unable to give any information with regard to the cause of the disease in this case, as it is impossible to ascertain anything definite concerning the influences to which he may have been subjected while in Philadelphia; but the fact that the cholera was prevailing pretty extensively there at the time is somewhat significant. Although several persons were exposed to the disease during his sickness, there were no cases following the exposure.

Reported by DR. B. F. CAMPBELL, *East Boston.*

No. 15. SEPT. 11.

M. G. L., a girl aged two years, resided in 38 Carroll Place. This child had a slight diarrhœa in the morning, early, and appeared a little depressed; slept an hour. At 11 o'clock, vomiting, purging and cramps commenced. The discharges were clearly rice-water in character, and continued, with the vomiting and cramps, until 1 o'clock, when I saw her for the first time. She was then pulseless, extremities cold, surface blue, lips livid, half unconscious, eyes sunken. In fact, she was in the stage of collapse, and died in fifteen minutes afterwards, or two and one-fourth hours from the time the vomiting and active symptoms commenced. The mother thinks she passed no urine during the morning hours.

A boy, four years old, living in the house adjoining, died four days before with the same train of symptoms. He was sick three or four days. The mother of the girl was with him every day during his sickness; and, the last day, her daughter was present, and about his bed most of the time. The houses where these children died are at the lower end of the Place, or Court. I did not see the boy during his sickness. His death was returned as cholera morbus.

Reported by DR. A. B. HALL.

No. 16. SEPT. 14.

Dr. A. A. Gould had been as well as usual and attending to his business, when about noon of Friday, September the 14th, he had

two watery discharges from the bowels. During the afternoon he had three or four more copious discharges of the same character. Between six and seven o'clock, vomiting began, and shortly before eight o'clock very severe cramps occurred in the feet and calves of the legs. About quarter past eight he had a very copious discharge, more than half filling the vessel, presenting the rice-water appearance, *but having some fecal odor*, and containing some small particles of fecal matter stained with bile. In the course of an hour these symptoms were gone, and the patient seemed quite comfortable and had a good pulse. He had no more diarrhœa, or cramps, or vomiting, except that he vomited a dose of Aqua Ammoniæ. There remained an unpleasant coldness of the hands, and from that time the patient steadily failed. At two, A.M., he was in collapse; the face and hands shrunken and livid, the skin cold and clammy, the radial pulse absent, the voice almost inaudible. He was restless and uneasy, took ether occasionally, apparently to relieve the uneasy and distressed feeling which accompanies collapse; and, notwithstanding the coldness of the skin, could not bear the encumbrance of the bed-clothes, even the lightest.

All efforts at restoration by the stimulants administered, both by the mouth and per anum, were without effect. He continued steadily to fail, and died at five o'clock, A.M., Sept. 15.

Reported by Dr. S. CABOT, JR.

No. 17. SEPT. 20.

About 9, A.M., on the 20th day of September, I was called to No. 17 Napier Street, to G. S., a teamster, thirty-seven years of age, and, by report, a man of intemperate habits. I found him in a small, dark, badly ventilated room on the lower floor. On examination, I could detect no pulsation in the radial artery, and the skin had a peculiar shrivelled appearance, and a leaden hue. I spoke to the man, and he answered me in husky tones, but seemed perfectly conscious and free from pain. I learned from his wife that he had had a slight diarrhœa for two or three days, to which he had paid little or no attention; and that very soon after returning from his work, on the evening previous to my visit, he became suddenly worse, and, to use nearly her language, "a large quantity

of whitish water passed through him, and very soon violent cramp and vomiting came on." My diagnosis was Asiatic cholera; and, never having had a case before, I called upon my friend Dr. James F. Harlow, who kindly consented to see the case with me. He coincided with me in my diagnosis, and we did what we could for the man, but he died before the next morning. I reported this as a case of cholera, appending to my report a note stating that the locality required the attention of the Board of Health. I considered that the tenements in that vicinity were over-crowded, and that some of the vaults and cellars needed attention.

I could not learn that the patient had been previously exposed to any case of cholera or choleraic diarrhœa.

Reported by DR. A. J. FENN.

No. 18. SEPT. 25.

Having suffered from a disagreeable, though nearly painless, diarrhœa (which I tried unsuccessfully to check by such moderate doses of Tr. Opii as would not interfere with my avocations), accompanied by lassitude and great nervous depression for ten or twelve days previous, I was taken suddenly worse on the evening of the 25th September. At first, sharp purging of fecal and watery matter; then sudden and violent vomiting, first, of unaltered ingestæ (a light supper of tea and dry toast), then of the characteristic rice-water fluid, which was expelled with much force, but without the slightest feeling of nausea, and without premonition. The first *characteristic* alvine discharge was very copious; afterwards (I think from the effect of the remedies used), though frequent (ten or twelve during the night), they were quite scanty. The cramps were not a prominent symptom, affecting principally the muscles of the calf, and slightly those of the abdominal walls. They were severe only at one time, and then for about three or four minutes. I suffered more from a sensation of burning heat, and from extreme oppression at the præcordia, than from any other symptoms. Pulse at one time forty-four, thready. No secretion of urine until the afternoon of the following day. When attacked, I was in my office and was unable to get home; and when I became aware of the danger of my condition, as no one sleeps in or remains in

the building after six o'clock, I tried to call a policeman to get me a coach, but was unable to, from complete aphonia; although I retained sufficient strength to reach the water closet, in the same room.

As to the treatment: When I became thoroughly alarmed, knowing that I was alone in this large building (Commercial Block), and remote from assistance, being convinced of the futility of trusting to small doses, I took at once Tr. Opii, ℥ ij., Ætheris, ℥ j., Tr. Capsici, gtt. l., Spt. Vini Gallici, ʒ iij., Aquæ, ʒ iss., M. This was retained for about twenty minutes, and I think had a most salutary effect. I had to repeat half the above dose four times, at varying intervals, and about three o'clock the cramps had entirely left me; the discharges ceased, and I slept until nine o'clock, from which time I began to recover, but for at least four weeks I was quite feeble.

With regard to exposure to any exciting cause, I would say that my office is in Commercial Block, a large granite building, very favorably located for sanitary influences (with two exceptions, stated below), fronting on the water, opposite the Eastern Packet Pier; office, up stairs, one story, and no dwelling houses in the vicinity. The light is as good as the ventilation, — the sun shining in the greater part of the day.

The two exceptions above mentioned are: first, in hot weather, for about two hours at low water, the mud of the dock basin opposite being exposed, and one (and I am not certain but two) of the common sewers discharging therein, when the wind is from the east and light, the smell is peculiarly oppressive and sickening. The other exception is the fact that the water closet, which is in the large apartment with which the office communicates, is used by the people on the lower floor, ship chandlers, and they often send their customers and other strangers, chiefly seafaring men, up to use it; and it is consequently, at times, somewhat of an annoyance, and *might*, haply, become a focus of infection, if used by any one suffering from cholerine.

Reported by Dr. James L. Williams.

No. 19. SEPT. 25.

Was called to Nathaniel Gardner, aged twenty-six years, a seaman, residing at his mother's boarding house, No. 368 Hanover Street. He had delirium tremens, but was about the house; had taken little or no food for several days. The delirium was not wild, and the patient manageable. A diarrhœa was upon him, attended by vomiting. Pulse irregular; skin warm; no cramps noticed or mentioned. Mixt. Calcis C. was ordered; opiate at night, and Valerian; a little brandy occasionally; coffee, toast and soup for nourishment.

SEPT. 27, EVENING. Was called again to G., fifty-six hours after first visit. I was told that the diarrhœa had increased after my visit, and the vomiting; that violent cramps supervened, and great prostration, with loss of voice. A physician in the neighborhood had been called in my absence, and passed the catheter, but obtained no urine, — and prescribed also.

The following appearances were present at this visit: General blueness of the entire body, with shrivelled and sodden skin, and cold; pulse hardly perceptible, and irregular; voice hoarse, and unable to speak above a whisper; no pain, and no dejection for several hours. The discharges, I was told, had been thin, free of odor and like rice-water, for the twenty-four hours preceding their cessation. No urine had passed for the day. The patient was thoroughly collapsed, yet perfectly conscious of his danger. Ammonia in Camphor water, and hot brandy and water were, prescribed. He died early the following morning, Sept. 28, about sixty-two hours after my first visit.

The patient had followed the sea from boyhood, and always been intemperate, and frequently had delirium when on shore. At such times, he was in the habit of almost totally abstaining from food. There had been no exposure, in his case, to cholera, that I could learn.

No. 20. SEPT. 30.

Visited Samuel G., brother of preceding, at his mother's, No. 368 Hanover Street, at 7 o'clock, P.M. Age twenty-eight years, a

teamster; intemperate in his habits, till within two months previous, when he took the pledge. The patient was lying on a mattress on the floor, skin cold and clammy, eyes sunken, hoarse, with frequent vomiting, and urgent diarrhœa, mind clear, complaining of severe cramps in body and lower extremities, thirst considerable, pulse rapid and weak, with cold tongue.

Patient stated that, on the morning of the visit, he had a slight diarrhœa, and ate cold veal for breakfast, which increased the trouble; and at dinner took ox-tail soup. The discharge of urine was slight during the day, and the stools thin and watery.

Nov. 1, — *Morning visit.* Patient passed a restless night; cramps very severe, but alleviated by inhalation of ether; discharges diminished, — examined, and found to be free from odor and like rice-water; no urine since last visit, and the bladder placid; complains of stricture about the heart.

Evening visit. Stools reported as diminishing, but a few in the afternoon, — watery, brown and offensive; no urine; catheter passed, but only a few drops obtained; cramps subsided, and slight warmth noticed about the abdomen and calves of the legs; pulse slight. The collapsed stage was well marked. Hoarseness and oppression of chest excessive; the entire body blue; feet and extremities cold and sodden. He continued to sink, and died early the following morning, forty-six hours after the commencement of diarrhœa, and thirty-four hours after my first visit.

The remedies employed were artificial heat, frictions with flannel and dry mustard, tincture of Opium, by the mouth and enema, Alkaline mixtures, Brandy, and inhalation of ether; and for nourishment, beef tea, gruel, coffee, etc.

His wife, with a nursing child, was in attendance throughout his illness. Immediately after his death, I was informed, she was seized with a violent nervous excitement; diarrhœa supervened, and she died in twenty-four hours after her husband's death. I did not visit her; but the case was recently reported as cholera, by Dr. Ryan, in the *Boston Medical and Surgical Journal.*

The stools were promptly removed to the cesspool, and the room kept as clean as possible, and well ventilated.

The patient lived in the lower part of North Street, in a filthy

locality; but was sick at his mother's, in Hanover Street, a very decent boarding-house for seamen and others.

This case I consider a thorough one of cholera, manifesting all the prominent symptoms of the disease as I have witnessed it before. I have been thus particular, that no important fact might be omitted.

Reported by DR. JAMES AYER.

No. 21. AUG. 30.

Was called, in the absence of their own family physician, to see Mrs. B. W., age forty-seven years, on Washington Street, near Roxbury line, at 10.30 P.M. I found her in a state commonly known as "collapse." Diarrhœa, rice-water discharges and vomiting, cramps, blue and shrivelled skin, suppression of urine, pulse difficult to detect.

This lady had not been exposed to any other cases of diarrhœa, or cholera, or cholera morbus. I think this patient was guilty of indiscretions, one of which was going to Jamaica Plains and back, after the looseness and pain in the bowels had commenced. She walked to and from Washington and Tremont Streets, to meet the horse cars, which fatigued her very much, and she rapidly grew worse on her return home.

Residence, highest part of Washington Street; drainage, good, but the walls of her apartments were wet from a leakage in the pipes above. She recovered, but her convalescence was slow.

Reported by DR. L. R. SHELDON.

No. 22. Nov. 3.

Nov. 3, 1866. Antoine DeLibues, Portuguese, aged thirty-three, seaman, at No. 10 Cooper Street. The patient was a stout, robust, muscular man; having been in Boston only eight or ten days. He came from the coast of Africa, where there was much sickness when he left. He was married and the father of two children. At one time he had chronic diarrhœa, for ten months; was subject to relapses from time to time.

Nov. 3. A diarrhœa with copious discharges commenced; after standing a few moments in the vessel, the discharges became frothy. On Sunday the 4th, he had rice-water dejections, vomiting and violent cramps. The vomiting and discharges were quite frequent, and the muscular contractions of the lower limbs very painful.

When I saw him, he was in a collapsed stage; pulse absent, extremities cold, skin bluish, great thirst, bladder empty, intellect clear.

Morphia in small doses, and bismuth, checked the vomiting, but not the discharges. Stimulants failed to restore the pulse.

Nov. 5. Condition of patient the same, except the vomiting. The temperature of the room was elevated to ninety-six and one hundred degrees. With this increased heat of the atmosphere in the room, and stimulants of hot brandy and water, with external use of mustard baths, the warmth of body gradually returned, and the patient ultimately recovered. During three days he did not pass any urine.

This man was sick about ten days. He was pulseless most of the time for the three days during the time when there was no secretion of urine. At no time was there aphonia or delirium. The heated atmosphere of the room did much to restore warmth and equalize the circulation. This man had indulged in some irregularity in eating, the night previous to his attack. He resided only a few rods from where the children died in Carroll Place.

Reported by Dr. A. B. Hall.

No. 24. Nov. 14.

Victor Augur, age twenty-five. [Case to which Dr. Townsend was exposed.] Birthplace, Taunton, Mass., of French parentage. Occupation, carriage-maker. Residence at time of death, 20 Salem Street. Date of death, Nov. 15, 1866.

The patient lived in a four and a half story modern brick dwelling. Location and surroundings good; no stagnant water or noxious vapors. Rooms light and airy. Condition of dwelling very clean, and the room in which he was confined, twelve feet long by eight feet wide, high studded, with one window, and perfectly clean.

Habits of deceased, sober and industrious; for a few weeks previous to his death he had worked from 6, A. M., to 11, P. M., at Whittier's carriage manufactory on Haymarket Square.

The deceased was in the habit of visiting the privy connected with the Boston & Maine Railroad depot, where hundreds visit daily, and which is furnished with Cochituate water, and drained by a sewer; but which, notwithstanding all precautionary means are used to keep it clean, is in a very unclean and unhealthy condition, owing to the habits of those who visit it. I was called to visit the patient between 12 M., and 1, P. M., Nov. 14, 1866. Found the patient in great distress; pulse one hundred and thirty, very weak, and easily compressible; breathing, very quick; face, with a very anxious expression; eyes sunken, lips purple, extremities cold and clammy. He was in great agony from severe cramps and pains in the extremities, but did not complain of any pain in the chest or abdomen. Slight nausea, but no vomiting as yet. Was having characteristic rice-water discharges every few minutes. The patient did not pass any urine during the time I was in attendance. On inquiry, found he had been complaining for several days previous to my being called, but had continued his labor up to the forenoon of the 14th, when, by the exhaustion of the excessive rice-water discharges, he swooned at his place of labor, and was carried to his boarding-house.

Finding the patient so far advanced in the third stage of cholera, I gave the following stimulant, being the first at hand, viz, Brandy and Capsicum in liberal doses, and then ordered an ounce of the following mixture, ℞ Gum Opii. ʒ ii. Aq. Puræ, Sp. Camph., āā ʒ vjii. Ol. Menth. Pip. ℥iv., Liq. Potass., ʒ ii. M. Dose, half a teaspoonful every fifteen minutes, continuing the Brandy and Capsicum in small alternate doses; brisk friction was applied to the whole surface of the body, particularly to the extremities.

In the course of forty minutes, I succeeded in producing considerable reaction. The spasms had ceased, and the patient was comparatively comfortable.

In the mean time Dr. McDonald, who had been previously sent for, arrived, and he suggested Sub-Nitrate of Bismuth and Sulphate of Morphine, with fractional doses of Calomel every hour,

which advice was followed; he also suggested Gum Arabic water, with Sp. Camphor, to be given in teaspoonful doses every fifteen minutes, in order to quench the thirst, which was considerable.

The patient remained in a very comfortable condition (the discharges having ceased and reaction having fully taken place) up to 6, P. M. At that time Dr. Townsend was called to take charge of the patient, and I have no personal knowledge of the symptoms of the patient after I left him. He died, as I subsequently learned, at five o'clock, A. M., Nov. 15.

I understand that the vomiting and purging returned in the evening; his attendants noticed him to turn over at one o'clock, A. M., as they supposed, to go to sleep; no one disturbed him until five o'clock, A. M., when he was discovered to be dead.

Reported by ELI THAYER, JR., M. D.

No. 25. Nov. 16.

The case of Dr. William E. Townsend, which possesses also a painful interest, was reported at the meeting of the Suffolk District Medical Society, held Nov. 29, by Dr. C. E. Ware, as follows:

Dr. Townsend had, during the past summer, been less well than formerly. He had not taken his usual vacation, and had had a good deal of night work. Friday, the 16th of November, he attended a funeral. Two days previously he had made a visit to a fatal case of "cholera." Dr. Ware was called to see him on Friday, at 5.30, P. M. He had eaten his dinner as usual. He had had no diarrhœa previous to his attack, but had felt somewhat feeble. The attack begun with vomiting and diarrhœa. When seen, he had had several dejections and vomiting, which did not yield to simple treatment. The discharges were watery, with marked fecal odor and color. He vomited immediately after a dose of opium, and then got, over the abdomen, a subcutaneus injection of Morphine, which gave no relief. The skin was warm and natural; the pulse accelerated, — half an hour afterwards almost imperceptible. An injection of meat tea, laudanum and brandy was administered, which was retained. Half an hour afterwards he vomited for the last time. Coldness began half an hour before the cessation of the diarrhœa, and gradually increased, and lividity came on. Pulse almost gone. Great faintness.

At 7.30, P. M. the pulse was entirely gone, and for an hour or two did not return. The patient took food and drinks—dilute Sulphuric Acid—and coffee, which he relished much. He was in this condition till—

9.30 o'clock, when there was a slight return of warmth, but no pulse. The lividity and faintness continued, though the latter was less marked. Then the pulse began to return. Mind, perfectly clear. Continued in this way through the night. Had two dejections, watery, with rice-water appearance, but having slight odor. No sleep. Pulse off and on.

In the morning at 8.30 o'clock, the pulse was constant, though very slight. No dejection after 3 or 4 o'clock, A. M. No nausea. Occasional cramps, not severe. Continued in this way during the forenoon. Urine not passed after 3, P. M., of the day before.

He was last seen at 11.30 A. M. Complained of asthma (to which he had been subject for many years), and was using a cigarette. His condition looked then as favorable for rallying as at any time; three-quarters of an hour afterwards he was dead.

On the twenty-ninth of January last (City Doc. No. 21, 1866), I had the honor to present to the Board of Health a communication upon the contagious nature of Asiatic cholera. The history of the disease, as it prevailed in the United States, subsequent to that time, and the almost universal concurrence of professional opinion in all parts of the world, has strengthened and confirmed me in the position I then assumed.

Professor Stille, of Philadelphia, writes:

"If there is any fact in the history of cholera diffusion established above all others, it is that no case of the disease can arise independent of preceding cases, and the whole weight of evidence goes to prove, that the disease is propagated by the excretions of the sick, and mainly by their infecting water which is used for drinking."

Dr. Anstie, of London, a gentleman well known to the profession as a sound writer and careful observer, remarks in his little volume on epidemics:

"On the whole, the practical conclusion which seems most consonant with all the facts as to the propagation of cholera is, that in the great majority of instances, the communication of the disease takes place by human means."

Dr. Jenner, President of the Epidemiological Society of England, at the opening of the session of 1866–67, in an address full of suggestions, worthy the consideration of every Board of Health, holds the following language with reference to zymotic diseases, so called:

"1. While we know nothing of the nature of the contagious or zymotic substance of any one of them, we do know that it is present in a special state of activity in different excreta in different diseases, — thus in the gastro-intestinal secretions in typhoid fever, cholera and cattle plague; in the skin and throat secretions in scarlet fever; in the throat secretion in diphtheria; in the pustular matter secreted by the skin in small-pox; in the discharge from wounds in pyæmia.

"2. We know that the special conditions of their surroundings have a great effect in favoring the spread of certain zymotic diseases, — thus a temperature of 32° destroys the poison of yellow fever; free dilution with fresh air renders the contagious matter of typhus innocuous.

"3. *Vehicle.* It is now placed beyond question by the labors of Dr. Snow, confirmed as they have been by experience, that water is one of the great agents in diffusing cholera; and, if possible, it has been more unequivocally proved, that typhoid fever is carried from individual to individual in the same way. Pyæmia, ichoræmia and septicæmia spread in wards by the morbid secretions of one patient being applied directly to some raw surface on another. The cattle plague contagious matter, being especially contained in the intestinal excreta, was conveyed by any substance to which that could adhere.

"4. *Individuals.* Whatever lowers the tone of the nervous system, renders men peculiarly susceptible to the action of the contagious substance of typhus, — e. g., famine, anxiety, fear. Per-

sons suffering from scarlet fever, and from Bright's disease, are in a state favorable to the occurrence of pyæmia. Those who have recently lived in a cold climate suffer more, certainly, than do those coming from hot climates, when exposed to the zymotic principle of yellow fever. Dr. Beale states that he has recently observed in fatal cases of cholera certain changes of structure in the intestine, which must have occurred before the attack itself.

"As to the surroundings, it has been clearly shown that many conditions, unequivocally removable, favor the spread of contagious diseases. Society has its part to perform in providing proper dwellings for the poor; i. e., dwellings proper in regard of air, light, drainage and water supply. Law can do much to improve surroundings by enforcing general drainage, and constant and sufficient supply of pure water. Great influence can be exerted in checking the spread of the zymotic diseases, by subjecting to strict supervision the special vehicles by which each is known to be conveyed from individual to individual. It is beyond dispute, that water has been in many cases the great agent in the rapid propagation of cholera and typhoid fever over a district. It becomes more probable with every searching investigation into local outbreaks of cholera and typhoid fever, that these outbreaks, so terribly destructive to life, are the result of the admixture of the zymotic element of the one or the other with the drinking water, or of collections of fetid gas, or of the close packing of the inmates of the house; and such close packing of the inmates of the house, and such close relations of the inhabitants to each other, that the excreta of the first person affected is taken into the stomach of the others."

"The great objects, then, in the prevention of the present mortality from zymotic diseases, are —

"To exclude or destroy the zymotic element.

"To improve the surroundings, so that the conditions may be less favorable for its development, and that fewer individuals who suffer, may die.

"To watch carefully the vehicles by which experience has shown the zymotic element may be conveyed to others.

"To render individuals less susceptible to the influence of the zymotic element.

"Numerous points in relation to the zymotic diseases have to be cleared up before we can reach that position in regard of their prevention which we may hope one day to attain.

"It is only by slow degrees that the profession has succeeded in separating some of these diseases from each other, — small-pox and measles, measles and scarlet fever, rubeola notha and rubeola; and now, for the prevention of the zymotic diseases, one of the most important problems in practical medicine has to be solved, namely, the identity or non-identity of several of these specific diseases with others which resemble them in some of their most prominent symptoms.

"Cholera is contagious: on this point there is no longer question. But what is the specific-cause relation between cholera and choleraic diarrhœa, and between severe summer diarrhœa and choleraic diarrhœa? [and, it may be added to Dr. Jenner's text, between cholera and cholera-morbus or English cholera?] Is cholera, in the form of choleraic diarrhæa, always amongst us? Again, the Cholera Conference considers it proved, that the evacuations of cases of cholera, so mild as to merit the name of choleraic diarrhœa only, can communicate cholera; as modified small-pox may be the most virulent form of that disease." — *Lon. Lancet*, Dec. 1, 1866, p. 599.

Other strong evidence bearing on the question of its propagation, and which tends to sustain the general assertion, that water is the great vehicle of cholera transmission, is to be found in the comparative immunity, during the past cholera season, which those cities of the United States have enjoyed, which are supplied with water, by a system which practically insures freedom from admixture with sewage.

I desire to call attention to that part of the quotation just made, in which Dr. Jenner remarks, that, "for the prevention of the zymotic diseases, one of the most important problems in practical medicine has to be solved, namely, the identity or non-identity of several of these specific diseases with others which resemble them in some of their most prominent symptoms."

It is well known that a disease, so very similar to Asiatic cholera as in extreme and fatal cases to be almost indistinguishable from it, has prevailed in this city as far back as the records in the City Registrar's office give the opportunity for investigation. Beginning with the year 1811, which was the first year in which a classified account was kept of the different diseases terminating fatally, we find:

Year.	Total Deaths.	Cholera Morbus.	Asiatic Cholera.
1811	892	6	0
1812	632	2	0
1813	750	3	0
1814	695	2	0
1815	830	1	0
1816	873	5	0
1817	875	6	0
1818	925	3	0
1819	995	11	0
1820	1014	6	0
1821	1304	9	0
1822	1188	5	0
1823	1040	2	0
1824	1208	7	0
1825	1362	11	0
1826	1167	6	0
1827	939	5	0
1828	1159	7	0
1829	1126	1	0
1830	1025	8	0
1831	1353	14	0
1832	1675	8	78
1833	1374	5	0
1834	1440	9	0
1835	1819	9	0
1836	1648	7	0
1837	1743	10	0
1838	1799	9	0

1839	1722	6	0
1840	1841	8	0
1841	1783	5	0
1842	2260	9	0
1843	2008	2	0
1844	2054	6	0
1845	2340	11	0
1846	3086	24	0
1847	3853	2	0
1848	3664	7	0
1849	5079	47	611
1850	3667	7	1
1851	3855	15	5
1852	3736	11	0
1853	4284	33	0
1854	4441	23	261
1855	4080	9	0
1856	4253	0	0
1857	3958	2	0
1858	3840	9	1
1859	3738	6	0
1860	4390	0	0
1861	3965	0	0
1862	4120	22	2
1863	4699	27	1
1864	5111	15	0
1865	4541	21	4
1866		46	11

Up to the year 1832, Asiatic cholera was unknown to the majority of the profession even by name. No doubt existed, then, and probably none exists at the present time, that the disease set down in the bills of mortality as cholera morbus, was a disease, or rather, in the language of another, a disorder, consequent upon the season, brought on by changes in temperature, errors in diet, or the result of some endemic cause. In this year a new

experience was opened to the profession. They were called to meet a disease, which, without any preliminary warning, killed as by a blow; defying treatment; and, marching with unseen tread, forbidding all trace of its footsteps. At its first visitation it numbered seventy-eight (78) victims. No more cases are reported, although the deaths by cholera morbus average about the same, until 1849, when, having entered the country the previous year, in its accustomed manner, it appeared in this city, and slew six hundred and eleven (611). In the same year, the deaths of cholera increased to forty-seven (47). In 1850, one (1) death is reported from cholera, and seven (7) from cholera morbus. In 1851, five (5) from cholera, and fifteen (15) from cholera morbus. In 1854, two hundred and sixty-one (261) from cholera, and twenty-three (23) from cholera morbus. In 1858, one (1) from cholera, and nine (9) from cholera morbus. In 1862, two (2) from cholera, and twenty-two (22) from cholera morbus. In 1863, one (1) from cholera, and twenty-seven (27) from cholera morbus. In 1865, four (4) from cholera, and twenty-one (21) from cholera morbus. In 1866, eleven (11) from cholera, and forty-six (46) from cholera morbus.

The accuracy of the reports previous to 1866 I have no means of determining, but with regard to those of the year just past this can be done. By the replies to the circular already alluded to, it was found, that, while only eleven (11) cases of Asiatic cholera were reported to the Registrar, and forty-six (46) cases of cholera morbus, many of the latter were characterized by symptoms which, in so far as the decision must be governed by a consideration of them alone, proved them to be Asiatic cholera rather than cholera morbus. And although there may be great doubt whether some of what, under the circumstances, and with our present ability of determining exactly between the two, it was thought most proper to call Asiatic cholera were not in reality cholera morbus of an extraordinarily severe type, there can be no doubt as to the fact that mortuary returns

given in this loose way are without the slightest value as a basis of vital statistics, except as showing how many have died in a given time. And this will appear in a still stronger light, when the fact is appreciated, that, although by a statute law a return must be made of every death by the physician in attendance, under a heavy penalty for refusing, no restriction is put upon the class of persons who may sign such return as the physician attending. It is not necessary that the party signing should be an expert at all. Any one can sign, and the Registrar has no authority to go behind the certificate. The returns, therefore, so far as they relate to the disease of which the party died, are uncertain and valueless.

In the last issue, for the year 1866, of the *Journal d' Anatomie et de la Physiologie*, some very interesting experiments are detailed, which go to show, that, under certain circumstances, not only can Asiatic cholera be communicated to animals by injecting the serum, the blood and the evacuations of cholera patients, but that a disease characterized by symptoms of cholera, as marked as when occasioned by the injections of the serum of cholera patients, can be produced by injecting *diastase*, — a substance produced during the germination of seeds.

"MM. Ch. Legros and E. Goujon, under the direction of Robin, at the Histological Laboratory of the School of Medicine, at Paris, with the serum, the blood and the dejections of cholera patients, succeeded in producing cholera in dogs and other animals. By injections into the veins, the trachea, and, in a few cases, by injections of considerable quantities of the liquid into the cellular tissue, all the symptoms of cholera were exhibited. The scene opened with vomiting, twenty minutes after the injection; then followed diarrhœa, and the expulsion of epithelial debris, cold extremities, labored and anxious respiration, suspension of urinary secretion, and other symptoms. A phenomenon which they have never known to fail, and which did not depend on the quantity of liquid injected, was a series of efforts at deglutition at the moment the liquid mixed with the blood. If the dog was vigorous and the

quantity of liquid injected diminished, he resisted the disease, warmed up, reactive fever set in, and there was a return to health.

"At the commencement, the urine was nearly always albuminous. If the dejections used were recent, clear and without color; or if the serum used were from blood drawn during the algic period, the results were very marked. When the liquid was old and colored, symptoms of putrid infection were joined to those of cholera, or replaced them. The blood of cholera patients, at the commencement of the disease, also produced marked symptoms when injected.

"An apparatus containing ice and salt, for condensing the vapor of the air in the cholera wards in some of the Parisian hospitals, enabled the experimenters to procure a liquid which, when injected into the trachea of dogs, determined similar symptoms to those of cholera.

"Injections of putrid substances failed to occasion such symptoms, though frequently causing diarrhœa and vomiting; but metastatic abscesses were found on post-mortem examination, which never occurred when the cholera liquids were used.

"Injections of Diastase, either pure or mixed with other substances, such as is obtained from crushed malt to which has been added its weight of warm water, produced symptoms of cholera as marked as when occasioned by the injections of the serum of cholera patients.

"MM. Legros and Goujon conclude: That cholera is a disease caused by a peculiar diastasic principle acting on the blood; and due to the presence in the air of substances containing this principle, which is absorbed by the lungs. That the presence of the diastase in the blood is a cause of the cholera symptoms. That injections into the veins or the trachea of animals of a liquid obtained by condensation in the air of a cholera ward will cause symptoms of cholera. That inoculation of the serum or dejections of cholera patients does not produce characteristic symptoms of cholera. That injections of certain quantities of the dejections or serum of cholera patients into the veins or trachea of dogs will cause cholera symptoms, and that large quantities of these substances introduced into the stomach of dogs will produce the same symptoms.

"That, when the dejections are old or putrid, they do not occasion cholera, but a putrid infection of the blood; that putrid substances injected into the blood tend to be eliminated by the intestine, producing diarrhœa, but not the symptoms peculiar to cholera.

"That the injection of diastase will produce the same symptoms as the injection of cholera liquids.

"That sporadic cholera is caused by the introduction with the food and drinks into the stomach of a certain quantity of diastase (a warning to drinkers of lager and other malt liquors).

"That to cure cholera, try to eliminate the diastase."

If, then, in the light of this uncertainty as to the identity of cholera and cholera morbus, as shown by the conflicting evidence thus presented, we have two diseases which resemble each other in very many points, but which are entirely different in character, one, the result of an imported contagion, and dangerous on account of its liability to be communicated from one to another; the other, sporadic, dependent on causes which, to a greater or less extent, are always in operation, and incapable of being communicated from one person to another, — it becomes a matter of the first importance to distinguish them from each other.

The assiduity of investigators, and the unusual interest shown both by professional and non-professional persons in collecting facts bearing on the ætiology of Asiatic cholera, has accumulated a mass of testimony greater probably than has before been brought to bear on a single disease. Indeed, the necessity of arriving at the truth in this respect has assumed such importance, that the European governments, early last year, united in sending delegates, diplomatic and medical, to a conference to be held at Constantinople, for the purpose of investigating the subject from every point of view, and in the most thorough and exhaustive manner.

The result of the deliberations of that eminent body may be summed up in the following extracts, taken from Professor Goodeve's lecture on the International Sanitary Conference, to which

he was appointed one of the Commissioners on the part of England, and the annexed portion of the Official Report of the British Cholera Commissioners.

"The Conference has concluded, that epidemic cholera originates entirely in India, and never in Europe; that it is a transmissible disease, following in the wake of man, and not carried by the atmosphere to long distances; that it is spread chiefly by the evacuations of cholera patients, and that it rages most in localities which are in bad sanitary conditions. It believes that the cholera poison may adhere to the surface of clothes, walls of houses, insides of ships, etc., and that these may be the means of communicating the disease to persons coming within their reach. Starting from these principles, it has based its measures of preservation, and asserts *that Asiatic cholera is to be met by measures of restriction of intercourse, when practicable; by measures of purification and disinfection; and by measures of hygiene.*" — *London Lancet*, Dec. 15, 1866, p. 659.

"Examples taken from the history of the present epidemic most strongly support the opinion of the great advantage of such measures. We may mention, that Sicily and Greece completely escaped the disease which was raging around them, in 1865. Sicily entirely cut herself off from all communication with diseased places. Greece caused all arrivals from infected localities to perform severe quarantine at four islands, — Delos, Pondiconyssi (Salamis), Skiatos and Vido, and held no intercourse with infected places.

"The good results of isolation in the cases of Sicily and Greece are hardly negatived by the examples of what occurred in other places said to be invaded in spite of restrictive measures. The quarantines enforced at Marseilles and some other ports of the Mediteranean were ineffective, either from their incompleteness, — or from their having been established too late, that is, after direct communication with infected ports had taken place.

"It seems to us that, in the case of ships or passengers arriving from infected neighboring ports, the following measures might advantageously be adopted:

"1. No persons should be allowed to land previous to efficient inspection by medical men appointed for the duty.

"2. The healthy passengers should be removed from the ship, and isolated for a period which need not exceed five days, at the end of which time they should be again inspected, and, if found without choleraic symptoms, should receive pratique.

"3. All persons with cholera or diarrhœa at the time of arrival, or at any period of the detention, should be isolated from the rest, and removed to a separate place. Cases of diarrhœa should be retained under observation until the diarrhœa is cured, or until the medical officer in charge is satisfied, from the features of the disease, that it is not of choleraic nature.

"We think that the time of observation in such cases of diarrhœa should not be less than eight days from the commencement of seclusion.

"Persons having a medical certificate of being sufferers from chronic or symptomatic diarrhœa should follow the rule of the healthy, — subject, however, to the discretion of the medical officer in charge.

"As the time occupied in the voyage between England and the neighboring ports is short, we have not included it in the period of observation.

"We further think that the complete disinfection of the effects of persons coming from contaminated places should be insisted on, and that the period of isolation of the persons should be from the time that they are separated from their suspected property.

"All persons (including medical officers) employed in the Quarantine Department who in any way come in contact with the ships, passengers, crews or effects that have arrived from contaminated places should follow the same rules as the arrivals themselves.

"With respect to persons detained in the sick departments of the quarantine stations, the destruction or disinfection of all articles used by them should be imperative.

"The application of chemical disinfectants to the discharges, the disposal of these below the surface of the soil, if on shore, and beyond the possibility of contaminating water used for drinking purposes, are indispensable.

"The above measures would require the following conditions at each quarantine station:

"1. An establishment for the reception of the healthy, capable of completely isolating successive parties of arrivals in distinct classes, well separated from each other.

"2. An establishment for the reception of the sick, with an isolated convalescent establishment.

"Each of the above should be provided with latrines, having movable receptacles, which should be daily emptied and purified.

"3. An establishment for the purification of effects.

"The establishments required would certainly be large, but a small number of them placed on a few points of the coast would suffice, if all ships carrying passengers from infected ports were made to pass through them before receiving free pratique.

"We consider that islands lying at some distance from the coast would be the most desirable spots for the institution of quarantine stations. On these, wooden — or, still better, iron—constructions might be rapidly raised. In summer weather isolated, camps with tents, might be formed.

"In the event of islands not being available, it would be well to select some place on shore capable of complete isolation, and at a considerable distance from any inhabited quarter; or hulks moored at some distance from the land, but never within rivers. It will be obvious, that several ships at each station would be necessary for the efficient working of the plans proposed.

"The principal of isolation, adapted to special circumstances, should, we think, be carried out within the country, when the disease has found a footing on shore.

"We cannot too strongly urge the necessity of excluding from workhouses and general hospitals any forms of choleraic disease.

"The sick poor should be cared for in special and isolated institutions."

(*Official Report of the British Cholera Commission.*)

In the Sanitary Branch of the British Army Medical Report for the year 1864 (*Army Medical Department. Statistical, Sanitary and Medical Reports, Vol.* 6, *for the year* 1864 — *Special Con-*

tributions — Report of the Cholera Epidemic of 1865 *in the Maltese Islands, by Surgeon Adams and Assistant-Surgeon Welch*), as the result of a careful survey of all the facts, it is reported "that the cause of this severe disease is an organic poison absorbed through the medium of one or more of the mucous surfaces brought into contact with external agencies, and reproducing itself in the human body, there can be little doubt. From all the well-authenticated cases we have been able to collect, the average interval between the exposure to the poison, and the onset of the symptoms was 3.4 days, and ranging from twenty-four hours to six days. A previous attack does not guarantee against its recurrence in the future.

"Looking more minutely into the facts furnished by this epidemic we are warranted in asserting that cholera may be communicated in the following modes:

"1st. By the atmosphere loaded with the virus emanating from the bodies of those affected by cholera or choleraic diarrhœa, through the medium of the ejections, dejections, respiration and perspiration. Among the military no less than thirteen were attacked by cholera while in hospital with other complaints, and no less than fifteen who officiated as nurses, and in whom the disease followed so quickly as to lead undoubtedly to the inference of infection from the sick one. We have several well-attested records of persons becoming affected with diarrhœa and vomiting on visiting infected places and persons. As an illustration we quote the following: Mr. Comyn, R. A., mentions a family living in great destitution in a wretched and exceedingly bad ventilated apartment, in which some of the members had died of cholera. The stench was unbearable; and, although the clergyman and himself were only a short time subjected to the atmosphere, both were seized immediately afterwards by nausea, vomiting and purging.

"2d. It is not requisite that a person should be laboring under symptoms of a choleraic character in order to convey the disease. There were many pointed facts to show, that, like scarlatina and other allied diseases, cholera may be introduced and communicated to susceptible persons by healthy individuals from infected districts.

"3d. By means of clothing saturated by the dejections, either in a dry or wet state.

"Among the military medical offices, there were few, prior to the epidemic, who had the least belief in the communicability of cholera, yet the evidence in favor of such a conclusion, under certain circumstances, soon became so strong and overwhelming as not to be withstood.

"Until late years the almost universal opinion with reference to cholera, was all one way. The disease was considered essentially mysterious in its mode of successive production, but in no wise communicable from person to person in any of the above methods. Accordingly some inexplicable and pestilential state of the atmosphere was supposed to be the only mode by which the disease could originate and spread, hence the utter uselessness of quarantine, or any measures for the separation of the healthy and the affected. As a matter of course, public fears and panics could derive no encouragement under such a belief, and, moreover, it agreed with the views of those who look on all like devastations as "visitations of God's wrath," and "His invincible decrees." However much good such doctrines may have done in quieting the public mind, and preventing a recurrence of the frightful scenes that characterized the earlier cholera epidemics, they were far from the truth, and have indubitably been the means of shutting men's eyes to a rational and scientific consideration of a disease, which, if not actually produced by man himself, has at least been fostered and kept in an active state by his culpable neglect of the study of the laws of health, and the conditions under which a beneficent Providence has placed him to live."

The report of that section of the Conference, to which the especial subject of "the Origin, Endemic Prevalence, Transmissibility and Propagation of Cholera" was referred, will be found in full in the Appendix to this Report. Its importance, in a sanitary view, has induced me to introduce it in this connection, and I have availed myself of the kindness of Dr. S. L. Abbott of this city, who has carefully and with great labor translated it, and placed it at my disposal.

From whatever point these facts on record are considered, the conclusion is irresistible, that to man and human agency alone is the progress of cholera, from one place to another, due; and that, just in proportion to the deficiency or completeness of the restraint which can be exercised over such agencies will be the spread or limitation of the disease. This, then, is the problem we have to solve: What kind of restraint will be most effectual in accomplishing this end? While it is indeed true, that, owing to certain geographical conditions, but little is to be hoped from sanitary cordons or police regulations in the Eastern Hemisphere, in our own country, it is not only possible, but, with united efforts on the seaboard, capable of practical effectuation. The length of the shortest transit, across the ocean which interposes between us and the Old World, is more than sufficient, according to all experience as to the time of its incubation, to develop Asiatic cholera on shipboard, and thus sound the warning note of precaution. The evidence on record shows, beyond a doubt or cavil, that cholera never attacks broadside, but always makes its entrance at isolated points on the seacoast. And not only this, but it has been repeatedly checked *in limine*, and extinguished at certain localities, as at Halifax, during the last season, and the Quarantine Station for the port of New York, under the stringent measures of the officers in charge. Indeed, so logical is the conclusion to be drawn from the results of exclusive measures, as witnessed in many parts of this country, both in the last and former visitations of Asiatic cholera, and as recorded abroad, that the assertion of Dr. Marsden, of Quebec, may be accepted as within the truth:

"*If our knowledge and experience be carried into practical operation, Asiatic cholera will become an unknown disease to future generations on this continent.*"

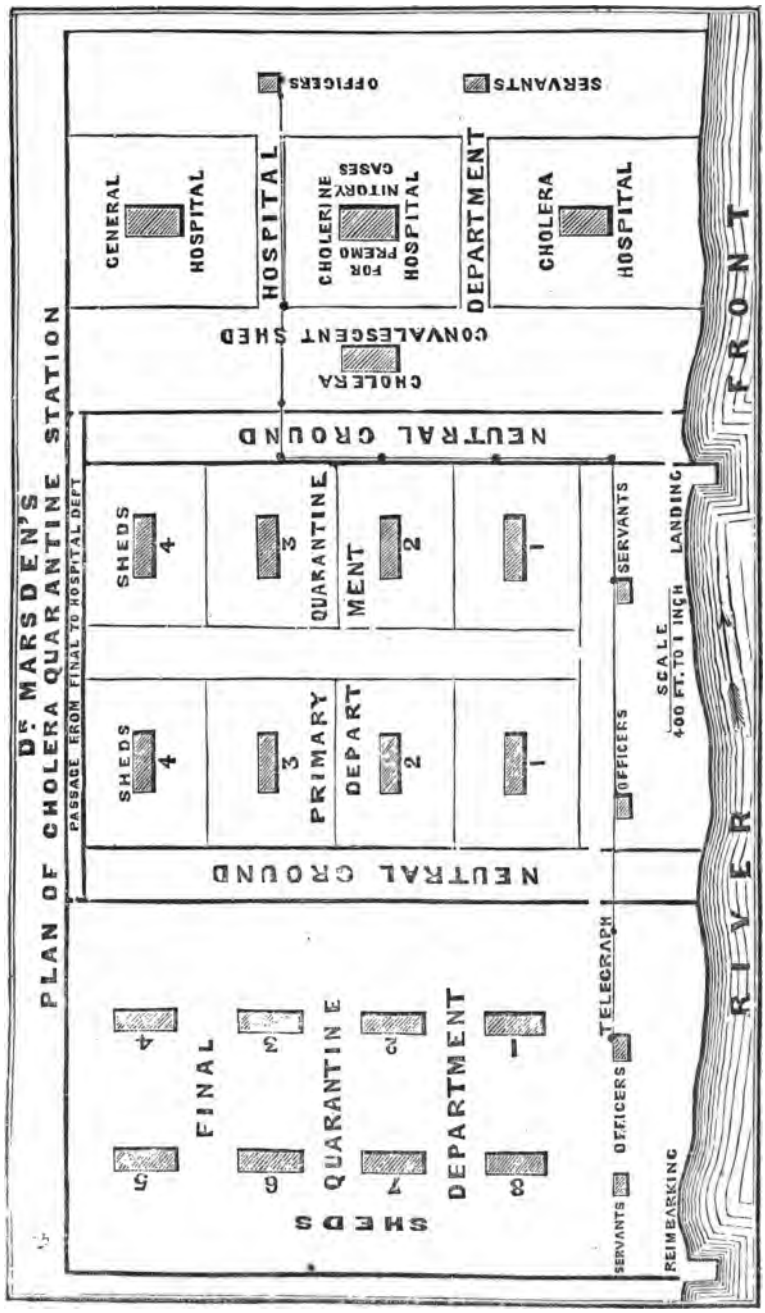
The prime essentials of every plan of restrictive intercourse are that it shall be complete, and capable of general application.

That which comes nearest, indeed, which seems to combine all the requisites for successfully accomplishing this object, is the one which has so often been brought before the community by Dr. Marsden, alluded to. It provides as well for the comfort of those who are subjected to its restraint, as for the safety of the community; and commends itself particularly to us, who, by the fortunate position of the islands of the harbor over which the city has control, are offered the best conditions for a successful quarantine. Dr. Marsden has kindly loaned me the materials for affording the City Government an illustration of his plan, which follows; and which is most respectfully submitted as a guide in whatever arrangements at any time it may become necessary to make with reference to Asiatic cholera.

The business of the office for the year ending Dec. 31, 1866, is as follows:

Persons vaccinated,	1606
Children examined, and certificates given for admission to public schools,	252
Physicians supplied with vaccinating material,	182
Number of visits made persons at the jail,	640
" " " " " " " Tombs,	8
" " " " " " " Temporary Home,	23
Soldiers examined, and certificates of disability given, under the direction of the Relief Committee,	636
Examination of persons appointed to the police,	35
Dead bodies examined by request of the Registrar, and certificates given,	3
Number of deaths from small-pox during the year,	49
" " cases of small-pox examined,	3
" " persons re-vaccinated at the jail,	230

Respectfully Submitted
M. Reed M. D.
City Physician


Dr. MARSDEN'S
PLAN OF CHOLERA QUARANTINE STATION
PASSAGE FROM FINAL TO HOSPITAL DEPT
SHEDS
FINAL
QUARANTINE
DEPARTMENT
NEUTRAL GROUND
SHEDS
PRIMARY
DEPART
MENT
QUARANTINE
NEUTRAL GROUND
CONVALESCENT SHED
CHOLERA
HOSPITAL
GENERAL
HOSPITAL
CHOLERINE
FOR
VOMITORY
CASES
HOSPITAL
DEPARTMENT
CHOLERA
HOSPITAL
OFFICERS
SERVANTS
TELEGRAPH
OFFICERS
SERVANTS
REIMBARKING
OFFICERS
SERVANTS
LANDING
SCALE
400 FT. TO 1 INCH
RIVER FRONT

DR. MARSDEN'S PLAN FOR A CHOLERA QUARANTINE STATION.

1. The Cholera Quarantine Station shall be divided into three separate and distinct sections or departments.

2. Each of these three sections or departments shall be isolated and separated from one another, by a *cordon* or portion of neutral ground, of not less than one hundred feet wide.*

a. One of these sections or departments shall be appropriated to the use of the sick, and shall be the Hospital Department.

b. The next or central section or department shall be devoted to the use of passengers not having had cholera, but from infected vessels.

c. And the third, or healthy section or department, shall be appropriated to the use of the healthy, who have been removed from the central department, after having performed quarantine there.

A. In the first section or department there shall be three separate and distinct hospitals, besides a convalescent shed or hospital.

a. The one for confirmed cases of cholera, to be called the CHOLERA HOSPITAL.

b. Another, for cases of choleraic diarrhœa, or other premonitory symptoms of cholera, to be called the HOSPITAL FOR CHOLERINE.

c. The third, for all other diseases, not cholera, or cholerine, but coming from on board infected vessels, or vessels having had cases of cholera on board, to be called the GENERAL HOSPITAL.

B. The next, or central section or department, shall be the Primary Quarantine Department, and shall be appropriated to all persons who are not sick, but come from vessels having had cholera on board, and wherein every case on landing shall undergo inspection, washing, cleansing and purifying, both of persons and personal effects. There a quarantine of four days shall be performed, at the end of which period of time all such persons as continue in sound health shall be removed to the Final Quarantine Department, and any that may fall sick, or be threatened with sickness, during the four days of probation, shall, as soon as detected, be removed to the proper hospital, in the Hospital Department. There, also, the healthy inmates shall be removed daily to a new locality, thus occupying four different habitations during their sojourn.

C. The third, or healthy department, shall be the Final Department, and shall be for all cases coming from the Primary Quarantine Department, after having been cleansed, washed and disinfected, and after having undergone the *four days* quarantine; and here a further quarantine of *six days* shall be performed (excepting cases coming from the convalescent hospital or shed, hereinafter provided for), making in all *ten days* of quarantine, when all persons continuing healthy shall be discharged from quarantine, and be removed from the station. If any premonitory symptoms or other cases of sickness occur in this department, during the six days of quarantine, they shall, as soon as discovered, be removed to the proper hospital, in the Hospital Department.

No communication shall take place with the Hospital Department, except through the central or Primary Quarantine Department, for which purpose a passage, unfrequented by the persons undergoing quarantine, shall be set apart and reserved.

The three sections or departments above described shall be designated and known as—

1. THE HOSPITAL DEPARTMENT.
2. THE PRIMARY QUARANTINE DEPARTMENT.
3. THE FINAL QUARANTINE DEPARTMENT.

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INTERNATIONAL SANITARY CONFERENCE.

REPORT

TO THE INTERNATIONAL SANITARY CONFERENCE, ON THE QUESTIONS OF THE PROGRAMME RELATIVE TO THE ORIGIN, ENDEMICITY, TRANSMISSIBILITY, AND PROPAGATION OF CHOLERA.

BY A COMMISSION COMPOSED OF MM. LE COMTE DE LALLEMAND, LE COMTE DE NOIDANS ET SÉGOVIA, DIPLOMATES, AND DOCTORS BARTOLETTI, BYKOW, BOSI, DICKSON, FAUVEL, GOODEVE, GOMEZ, BARON HÜBSCH, LENZ, MACCAS, MILLINGEN, MONLAU, MÜHLIG, PÉLIKAN, POLAK, SALEM, SALVATORI, SAWAS, SOTTO, I. SPADARO AND VAN-GEUNS.

DR. A. FAUVEL, *Secretary-General.*

[*Translated by* S. L. ABBOT, M. D.]

GENTLEMEN, — Before laying before you the results of its labors, the Commission thinks it advisable to inform you of its mode of procedure, in order that you may appreciate the care which it has taken to elucidate the numerous questions submitted to its examination. The Commission was organized by the election of Dr. Bartoletti as President, and M. the Count de Noidans and M. the Baron Dr. Hübsch, as Secretaries. In order to facilitate its labors, the whole Commission was then divided into six sub-committees, or sections, each having a distinct duty.

The first section, composed of five members — MM. Segovia (*President*), Goodeve, Polak, Van-Geuns, and Pélikan (*Secretary*) — was charged with answering the questions, comprised in the first group of the programme, on the origin and genesis of cholera; that

is to say, it had to elucidate the important and arduous questions of the endemicity and epidemicity of that disease in India.

The second section, consisting of MM. De Lallemand (*President*), Hübsch, Pélikan, and Mühlig (*Secretary*), had to investigate the facts relative to the importation and transmissibility of the disease. Its field of study comprehended the first three paragraphs of the second group of questions.

The third section — MM. Sotto (*President*), Monlau, De Noidans, Sawas, and Maccas (*Secretary*) — was to study more particularly the circumstances of transmission: how, by what means, the cholera can be transmitted; what are, in a word, the agents of its transmission.

In addition, this section had to investigate the question of immunity, considered in relation to certain countries, to certain localities, to certain individuals residing in the midst of a cholera district.

The fourth section — MM. Gomez (*President*), Fauvel, Salem, Lenz (*Secretary*) — had assigned to it the duty of showing the influence of large collections of men, both on the violence of cholera epidemics and the propagation of the disease; to consider this influence on shipboard, in lazarettos, in armies, at fairs, in pilgrimages, — in fine, in all gatherings of human beings. On the other hand, its business was to determine the influence of dissemination from every point of view.

It was also its duty to consider hygienic conditions in connection with the causes of cholera epidemics.

The fifth section — MM. Millingen (*President*), Dickson, and Bosi (*Secretary*) — was to sum up all the facts obtained, and to deduce from them, so far as possible, a theory of the properties of the generative principle of cholera, with special reference to Prophylaxia.

Finally, the sixth section — MM. Goodeve (*President*), Bykow, Salvatori, and Bartoletti (*Secretary*) — was to give a general view of the course and propagation of cholera in 1865.

This simple enumeration, which gives an idea of the importance of the numerous questions to be solved by the Commission, sufficiently explains why nearly two months have passed from the first

meeting to the complete accomplishment of their task. During this interval, the Commission has devoted no less than nineteen full meetings to the discussion of the partial reports elaborated by the sections. All the questions proposed were there debated with the greatest care, without prejudice, or any other determination than to arrive at the truth by a rigorous deduction from the facts. The Commission has never lost sight of the practical object of its studies; it has also excluded from its debates all mere theoretic speculation; it has not forgotten that its labors were to serve as a basis for Prophylaxia; and, as it is convinced that the greater part of the conclusions which it has arrived at may lead in practice to the application of measures of great importance, it regrets neither the time nor the labor which it has devoted to establish them on a firm foundation.

The Commission was not contented with the first elaboration, necessarily somewhat incoherent, which resulted from the partial reports; it wished that all these scattered elements might be united, co-ordinated and condensed into a general report, which should be the expression of its judgment, and to which the partial reports, with their appendages, might serve as confirmation.

It is this joint work, adopted by the Commission, that it has the honor to submit to the Conference, — a work which, as it is now presented, is the result of three successive discussions.

The Commission therefore does not fear the reproach of having given its opinion lightly and without sufficient examination; it is conscious of having proceeded with all proper deliberation; and, if it has not been able to solve all the problems submitted to its consideration, it is because the actual state of our knowledge does not permit it.

FIRST GROUP OF QUESTIONS.

The Origin and Genesis of Cholera; the Endemicity and Epidemicity of this Disease in India.

If the Commission had been able to solve all the problems of this first group, it would certainly have accomplished the principal object of the Conference, which is to search for the origin and the primordial causes of cholera, in order to arrive at practical means

of circumscribing it, and strangling it at its point of departure. Unfortunately, such is not the fact. The Commission has been able, undoubtedly, to answer categorically many of the questions proposed, but those which it was most important to solve are still undecided, for the want of sufficient documents. Nevertheless, while still in doubt on obscure points, the Commission has endeavored to present, in a more precise form than has been heretofore done, the terms of a problem, the solution of which interests the whole world; and, in so doing, it believes that it has done a work of great utility.

At this time, the Commission would give notice, that it has not confined itself either to the order or to the letter of the questions inscribed in the programme, discussion having shown the necessity of introducing certain modifications, for the purpose of giving more clearness to its statements.

I.

Where did the cholera, known as Asiatic, originate? And in what countries does it exist in our day, in an endemic form?

In order to answer the first of these two questions, the Commission has not thought it necessary to undertake new investigations, to determine whether the cholera which we see in our day, is a recent or ancient disease; all that we shall ever be permitted to know on this point has probably been said.

It is beyond a doubt, that, long before 1817, and even at an epoch so far back as the first establishment of Europeans in India, there has been observed in that country, and in some of the neighboring countries, a disease bearing the greatest resemblance to the cholera of our epoch, and raging at times under the form of violent epidemics. Thus, in the sixteenth century, a well-known Portuguese physician, Garcia da Horta, pointed out the existence, in India, of a disease called *Mordechim*, or *Mordexim*, which, according to his description, was no other than the cholera (*communicated by Dr. Gomez*). But, not to go back beyond the period of which we have very exact knowledge, it is sufficient to call to mind, that, during the last part of the eighteenth century (1781, 1783 and 1791), many very deadly epidemics of cholera occurred in different parts

of India, and that in provinces, sometimes, very remote from each other. Such was the epidemic observed in 1783 at Hurdwar, in the north of Hindostan, and that which occurred at about the same time, at Travancore, in the southern part of the peninsula.

But what is equally certain and important to be noticed is, that, from the end of the last century, (whether it was that the disease was entirely extinct, or, rather, because it had escaped the attention of physicians on account of its trifling importance,) there was no question of epidemic cholera in India, nor elsewhere, until 1817. It is also certain, that the English physicians (Dr. Titler and others) who first found themselves in the presence of the epidemic of Jessore did not recognize it at first as the cholera which they had ordinarily met with in a sporadic form, and believed that they had to deal with a new disease, — a circumstance which tends to give the impression that in reality the new cholera differed in some respects from the old.

However this may be, whether the disease of 1817 was identical or not with that of preceding epidemics, it is certain that from that time dates a new phase in the history of cholera. Instead of remaining, as formerly, circumscribed in the provinces where it had appeared from time to time, under the form of epidemics, which died out at the place of their origin, cholera took on, all of a sudden, an invading character.

It extended gradually in all directions, and in a short time became general in the greater part of India, presenting, in its extensive march, transient remissions. Soon it passed the limits of this region, not merely in a single direction, but by all the outlets which gave passage to human currents.

For many years this outflow of cholera from India was repeated. The greater number of these currents, flowing in all directions, died out on the way; but at last, by the northwest, the disease found a passage, and made its appearance for the first time in Europe, in 1830.* This epidemic, after many years of ravage throughout the world, was extinguished wherever it had penetrated, with the excep-

* In 1823, the cholera appeared for a short time in Astrachan.

tion of India, and perhaps also some of the adjacent countries; but in India itself, from the date of 1817, cholera remained permanently. From that time, careful observations and authentic documents show its constant presence as either an endemic, in certain localities, or in the form of epidemics more or less general, sometimes in one province, sometimes in another, — epidemics recurring often after very short intervals. It was no longer, as before, a disease comparable, in ordinary times, with the *cholera nostras* of our countries, and taking on at long intervals the form of limited epidemics; it was an affection, in certain respects new, with foci of emission always active, — permanent foci, from which the disease radiates and propagates itself at a distance, under the form of invading epidemics. It is especially important to bear this in mind; for it is this which gives to the cholera of 1817 a very particular interest for us.

What is the practical result from this new state of things in India? It is, that, owing to this fact of a permanent focus and emanations from it, the neighboring countries, on account of their proximity to India, have been the theatre of repeated epidemics of cholera; and that twice also, in our time, the disease has laid out for itself a route as far as Europe; not, as has been claimed, by following a fatal direction, but by following those paths which presented themselves as most favorable for its passage. It was in this way that in 1847 it penetrated into Europe, at the same time, by the Caspian and Black Seas, while at the south its course was stayed in Mesopotamia and the Hedjaz. It was the same in 1865, and this time owing to the means of rapid transportation. It arrived in a very short time, by the shortest route, at the basin of the Mediterranean.

From this simple history, based upon an attentive study of the facts, flow two things, which, moreover, are not contested: that the cholera, from which Europe has suffered on three different occasions, has had its point of original departure in India, properly so-called; and that, since 1817, that country has been the constant focus from which the disease has radiated in all directions. Therefore the whole Commission is able to answer, without hesitation,

that *the Asiatic cholera, which has at different times run over the whole world, has its origin in India, where it had its birth, and where it exists permanently as an endemic.*

Adopted unanimously.

II.

Out of India, does the Asiatic cholera exist in our day, in any part of the world, in an endemic form?

Although no fact has been brought forward, up to the present time, to show that Asiatic cholera has ever taken its departure from any other point than India, and although it is probable that in no other country does it exist as a permanent endemic, capable of giving rise to invading epidemics, the Commission yet believes that it can show a difference between the countries bordering on, or in the neighborhood of India, (although with regard to these there do not exist sufficient data to pronounce positively,) and those countries where it is incontestible, that cholera has always come from without.

In the first category are found, Indo-China, China, the islands of the Indian Archipelago, and, nearer to us, Afghanistan, Beloochistan, Persia, and the eastern and southern side of the Arabian Peninsula. In fact, for fifty years, these countries have been the theatre of very frequent cholera epidemics, which, without doubt, are very easily explained by repeated importation from India, — and the Commission admits, that very probably this has been the cause; but, since it is asserted that endemic cholera has shown itself recently in certain regions of India, for example at Cawnpore and Allahabad, where it has not existed heretofore, and as the same thing may occur elsewhere, the Commission, in the absence of sufficient data with regard to these countries, thinks proper to remain in doubt.

It is not so with regard to Europe, the Caucasian Provinces, Turkey in Asia, the whole of the north of Africa, and the two Americas; in all these countries, the Commission does not hesitate to declare, that Asiatic cholera, *invading* cholera, has never had its birth. It is impossible to cite a single epidemic of this kind which has had its origin at any point of these countries. On the other

hand, all the epidemics of cholera which have been observed in these countries could always be traced, so to speak, from station to station, and, by tracing them to their source, followed back to their Indian origin.

Undoubtedly, it has not always been possible to follow without interruption the chain of facts, and there are cases which prove, that, when once the cholera is planted in a European locality, at St. Petersburg for example, it can maintain its hold for many years; but, as it is not our purpose here to discuss the mode of propagation, nor the causes which favor the continuance of the disease, we confine ourselves at the present time to the two foregoing considerations.

It is hardly necessary to say, that this question has nothing to do with the disease commonly designated under the name of *cholera nostras;* which, although ordinarily sporadic in our countries, may also, occasionally, and in the warm seasons, take on an epidemic form. It is sufficient, to fix the distinction, to remark, that this disease, under whatever form it shows itself, neither generally occurs of the same severity, nor with the same set of symptoms as the Indian cholera; and, further, — a distinctive, fundamental character, — never has an epidemic of this kind become a *propagating focus* of the disease.

Therefore the Commission considers as demonstrated, that the Asiatic — the invading cholera, wherever it appears, is never spontaneously developed, and has never been observed as an endemic (care must be taken to distinguish secondary foci, more or less tenacious in their character) *in any of the countries which have been enumerated* (Europe, etc.), *and that it has always come from abroad. As for the countries in the neighborhood of India, while admitting it as probable that the cholera does not exist there as an endemic, the Commission does not feel itself authorized to come to any formal conclusion on the subject.*

Adopted by all the members of the Commission, except MM. Polak, Sawas and Van-Geuns.

III.

Is there not reason to fear that the cholera may acclimate itself in our countries?

The answer to this question must be doubtful. Indeed, if we consider that the epidemic which came into Europe in 1847, remained a much longer period than the preceding one, and gave rise, in some localities, as at St. Petersburg for example, to secondary foci of considerable duration, it would seem that repeated invasions may be capable of acclimating the principle of the disease to some extent; but, as we know not yet what opinion to hold on this point with regard to the countries bordering on India, with so much the more reason we are not authorized to admit, that it will be necessarily so *in our countries.* For this reason, *the Commission, without rejecting the possibility of the fact, regards it as problematic.*

Adopted unanimously.

IV.

Is there in the Hedjaz an original focus of cholera, permanent or periodic?

The Conference having decided, that, in view of the particular importance attached to it, the question of cholera in the Hedjaz should be the subject of special investigation, it has been proved, in the first place, that celebrated travellers (particularly Niebuhr and Burckhardt), who visited this country before the invasion of 1831, make no mention of epidemic cholera, although they speak explicitly of the diseases which prevail there. Farther, it appears certain, that before that invasion there did not exist in the Hedjaz even a name applicable to epidemic cholera.

On the other hand, it is proved by numerous documents, published or unpublished, that, since 1831, epidemic cholera has made its appearance frequently in the Hedjaz: as (to cite only those epidemics about which there is the most authentic information) in 1835, 1846, 1847, 1848, 1859 and the following years, until the great epidemic of 1865;* with this peculiarity, however, that, during the

* It is proper to observe, that the presence of the cholera at Djeddah every year, at

six consecutive years, from 1859 to 1864, the disease did not prevail extensively. To these facts, if we add, that the appearance of cholera in the Hedjaz has always coincided with the time of the pilgrimage; that it is the general opinion of the country that it is always imported by pilgrims coming from India; and, finally, that, on many occasions, and most notably in 1865, it is positively asserted, that arrivals from India reached the Hedjaz affected with cholera before the disease had manifested itself there, we arrive at this conclusion, which is that of the Commission: *that Asiatic cholera does not appear to have in the Hedjaz an original focus, but that it appears, up to the present time, to have been always imported from without.*

Adopted unanimously, except by Mr. Goodeve.

V.

Are there in India certain localities which have the exclusive privilege of generating cholera, or which are particularly favorable to its development? In other words, is cholera endemic in all parts of India, or only in certain regions which it is possible to circumscribe?

It is well known, that, in India, cholera does not manifest itself everywhere with the same frequency or in the same manner. Observations made upon this point have established the following distinctions:

Cholera reigns by preference as an *endemic* disease, with a tendency to assume at certain periods an epidemic character, in Bengal in general, but above all in the city of Calcutta; and, with less intensity, at the stations of Cawnpore and Allahabad and their environs; and, in other parts of India, at Arcot near Madras, and Bombay.

It appears as an *epidemic* disease *every year, or almost every year*, with more or less violence, in the towns of Madras, Conjeveram, Pooree (Juggernath), Tripetty, Mahadeo, Trivellore, and other places where there are gatherings of Hindoo pilgrims.

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the time of the return of the pilgrims, from 1859 to 1864, is attested by a report by Mr. Stanley, the English Consul at Djeddah for six years, and is described also, so far as regards 1864, by M. de Ste. Marie, Consular Agent of France in the same town.

It appears also as an *epidemic*, but at uncertain periods, the intervals between which do not exceed, for the most part, four or five years, in the provinces of the Northwest of Hindostan, — in 1845, 1852, 1856 and 1864, — as well as in all parts of the Presidencies of Madras, Bombay, and in Pegu.

It results, then, from these facts, that cholera is not endemic, except in a very limited portion of India, — above all in the valley of the Ganges, properly so-called; and that all the other parts of that vast country are, in regard to cholera, under the same conditions as extra-Indian countries, — that is, the disease appears only accidentally, and as an epidemic, under the influence of causes more or less appreciable.

But, in order that these facts should have all their importance, and lead to a practical result, it is necessary that they should be established with precision. Now this is not the case. To say that the cholera is endemic in the valley of the Ganges and in the delta formed by that river and the Brahmapootra, without designating more precisely than has yet been done the points of this vast territory which have the privilege of endemicity, is to leave the question in a state of vagueness from which nothing positive can be concluded. The Commission hoped to receive official documents which might clear up this capital question in the etiology of cholera; but it has not received them, — without doubt, for want of time to prepare them and send them from India. It can only therefore indicate this important hiatus.

It would not be enough even to know with precision the localities where cholera reigns to-day permanently: it is necessary also to know if there are places where the disease has never failed to exist since it has been studied; if there are those where the endemic has disappeared, to reappear afterwards; and, finally, what are exactly the localities where the endemic is a fact relatively new as well as that it really exists. Further, is it entirely certain that cholera is not endemic except in circumscribed localities which have been pointed out; and is there not reason to suspect that it may exist also in certain of those places of pilgrimage where each year cholera takes on an epidemic development?

From the reply, more or less categorical, to these questions

might result the indications by which to arrive at the conditions of the genesis of the disease, and the prophylactic means by which to oppose it.

Nevertheless, an exact knowledge of the endemic foci would be still insufficient; it is important to have also that of the principal epidemics which have reigned in India since 1817, with a determination as exact as possible of their point of departure, in order to see if these epidemics have had, or not, their origin in an endemic focus, or by the arrival of parties from that focus. It is probable that on this point the Indian archives could give decisive information. Finally, it would be interesting to know if, in India, there are regions or localities which have, up to the present time, shown themselves refractory to the propagation of cholera.

By the aid of all these facts, we should, perhaps, be permitted to establish what we already may suppose to be the case, namely, that in India there exists but a small number of endemic foci of cholera, from which have gone forth the epidemics which have ravaged that country first, and afterwards the whole world. For the moment the Commission can only answer, *that there are in India certain localities, comprised principally in the valley of the Ganges, where cholera is endemic, without its being possible to indicate them all, or to affirm that they have the exclusive privilege of giving birth to the disease.*

Adopted unanimously.

VI.

Do we know the causes by the concurrence of which cholera originates spontaneously in India, as well as the circumstances which make it take on an epidemic character?

If it is incontestible that cholera does not exist in India in an endemic form, except in certain circumscribed localities more or less well determined, it is there, and not elsewhere, that we must seek for the circumstances which can give birth to the disease, or, if the expression is more satisfactory, produce its endemicity. Now, whether it is because no thorough research has ever been made on this subject, owing to the difficulty of the problem, or some other motive, it is certain that these circumstances have not,

up to the present time, been stated in a precise and satisfactory manner.

Hypotheses, nevertheless, have not been wanting. The principal one consists in attributing the endemicity of cholera in Bengal to the alluvium of the Ganges and the Brahmapootra, — an alluvium rendered more particularly deleterious, under a scorching climate, by the fermentation of animal and vegetable detritus, with which the soil is impregnated. Under this supposition, cholera would be the consequence of certain emanations from this marshy soil. It is added, that the custom of the Hindoos of abandoning to the current of the sacred river their half-burnt corpses may explain the privilege of endemicity which the delta of the Ganges possesses. Finally some persons, and, among others, Dr. Bonnafont (*Le Cholera et le Congrès Sanitaire, Paris*, 1866), believe that they can account for the permanence of cholera in India, and the greater frequency of epidemics, since the close of the last century, and especially since 1817, by the ruin of the great hydraulic works constructed by the ancient rulers of the country, for the distribution and easy flow of water for purposes of irrigation; a ruin, which, according to certain extracts from a work published more than twenty years ago, by the Count de Warren and others, from an English journal of the same epoch (*India News*, 1844), was due to the neglect of the East India Company, and has for its consequence the insalubrity of regions, up to that time, comparatively healthy.

All these assertions, which tend to nothing less than the determination, in a precise manner, of the cause of cholera in India, and which appear to enjoy a certain credit in Europe, have been the subject of an attentive examination on the part of the Commission.

To the hypothesis which attributes the genesis of cholera to the alluvium of the Ganges, the Honorable Mr. Goodeve, to whose word a long residence in India gives much authority, — Mr. Goodeve replies, that, in India, other rivers besides the Ganges present similar alluviums, without the prevalence of the cholera on that account on the soil which they water; that such is particularly, in a neighboring country, the Irrawaddy, — an immense river, of which the delta, nevertheless, has not the same privilege as that of the Ganges, and where cholera does not appear, except from time

to time, as an epidemic; that, without doubt, the alluvium of the Ganges is to Bengal, as is the case everywhere else, a great cause of insalubrity, which may, up to a certain point, favor the manifestation of cholera, but does not explain its origin. As for the influence of dead bodies, abandoned to the current of the river, Mr. Goodeve, in agreement on this point with M. de Valbesen, the late Consul-general of France at Calcutta, sees in them a morbific influence, the importance of which has been greatly exaggerated. And, moreover, it must not be forgotten, that this custom has existed from time immemorial, while the permanent existence of cholera on the banks of the Ganges is a new fact.

Finally, with regard to the explanation based upon the assertions of M. de Warren, Mr. Goodeve declares that it is entirely contradicted by facts. In the first place, the canals of which he speaks had been destroyed, or had ceased to be used, long before the British rule in India; next, they exist principally in the Carnatic, in the southern part of the peninsula, and not in the delta of the Ganges and the Brahmapootra, where the cholera of 1817 took its birth. This delta has never had these hydraulic works, and its streams have flowed through it for ages under the same conditions. Those who believe it possible to render this region healthy by works of this kind would do well to study the question on the spot. They would see, at the time of the great rise, in September, with what power the large and deep rivers, fed by a multitude of affluents, flow towards the sea by a great number of mouths, over an extent of more than a hundred miles of surface. They would see everywhere the slight elevation of the country, and the immensity of the territory to be drained; and then they would comprehend, perhaps, the difficulties of such a work, to obtain, besides, a result very problematic, — to wit, the extinction of cholera, which would more probably be obtained by hygienic measures applicable to the manner of living of the native population. It is in this direction, adds Mr. Goodeve, that the three permanent Sanitary Commissions in India are already working.

With regard to the destruction of the canals and the embankments, Mr. Goodeve demonstrates, in an article in the *Edinburgh Review* of January 1864, (an article based upon official documents),

that, in the Carnatic and the north of Hindostan, the destruction spoken of dates back, in the first place, to the decadence of the great Mussulman Empire; and, next, to the wars of the Mahrattas against that empire, — and consequently, long before the British rule, which merely took possession of the ruins already existing. As for the canals of the north of India, and those of Delhi in particular, they have not been employed for any useful purpose since the middle of the last century; and far from having been ruined by the neglect of the English, those of Delhi have been restored and extended under their administration by the works commenced in 1808, and finished in 1822. In every province the English have commenced anew the construction of canals, and all that they can be reproached with is, that, absorbed with war and politics, they have proceeded with an imprudent tardiness in such a series of productive and beneficent improvements. Among the great canals constructed by the British government may be mentioned those spoken of above, the Eastern Jumna Canal, having a length of one hundred and fifty miles in its principal course, and five hundred miles of branches, watering 58,287 hectares, and the Western Jumna Canal, which has a principal course of four hundred forty-five miles, without counting the branches, and which has given fertility and life to the whole of a vast country, until then sterile and depopulated. But above all should mention be made of the Canal of the Ganges, a gigantic enterprise, exclusively British, of a principal length of 1,437 kilometres, and to which, in the month of May 1862, were added 2,963 kilometres of trenches, without counting the secondary conduits. Mention should also be made of the great works by which the principal rivers of the south of the Presidency of Madras have been improved in their course, and turned aside for purposes of irrigation, by works commenced in 1836, — consequently before the time when M. De Warren published his book.

By this simple history, which might have been extended still farther, Mr. Goodeve believes that he has demonstrated — and the Commission concurs with him in opinion — the injustice of the opinion which charges the East India Company with having allowed to fall into ruin the ancient hydraulic works, and with having done

nothing of this kind for the improved health and fertilization of the country. The truth is that many years before the publication of M. De Warren's book, hundreds of miles of canals had been undertaken and restored in the Presidency of Madras and in the north. Finally, Mr. Goodeve thinks that he has proved, — and the Commission agrees with him — that the development of epidemic cholera in India should not be attributed to new conditions of insalubrity, due to neglect on the part of the English government.

The Commission has not thought it its duty to dwell upon other hypotheses, which would explain the origin or the endemicity of cholera by hygienic conditions which exist in the same degree as well in those parts of India where endemicity exists as in those where it does not exist. The moment that it is declared that the endemic existence of the disease is peculiar to certain parts of India, although all these places have not yet been precisely determined, and that it can be proved, besides, that this circumstance is a fact in some respects new, — *it follows, from the necessity of the case, that this cholera, permanently established but recently, should be referred to some new and special circumstance in these localities.* Now, as has been said above, no new or special circumstance has yet been discovered in the delta of the Ganges since the cholera has reigned there in an endemic form. And yet this is the whole question. *The permanence of the disease in certain localities should not be explained by successive transmissions, but only by something inherent in the places themselves.*

As for alluvial formations, climate, hygienic habits, misery, etc., it is evident that all these morbific causes should not be invoked in this place, except as favoring conditions.

The problem of the genesis of cholera cannot then be solved except by an attentive and patient inquiry at the places where it exists as an endemic. Certainly it is impossible to predict what will be the result of this inquiry, nor whether it will ever succeed in solving the problem; but it may be affirmed, that from it will always result something useful with regard to prophylaxia against cholera.

In the mean time, the Commission must limit itself to answering, that *we know not the special conditions under the influence of which*

cholera originates in India and prevails in certain localities as an endemic.

Adopted unanimously.

VII.

What are the circumstances which concur in the development and the propagation of epidemics of cholera in India?

As for the second part of the question, to wit, What are the circumstances which, in India, concur in the production of epidemics of cholera? we are in a more advanced position. Undoubtedly all the problems relating to its epidemicity in India are not yet solved, but the knowledge which has been obtained permits us to indicate a certain number of circumstances of which the action is incontestible. It is at the sources or in the immediate neighborhood of endemic foci, that we ought, it would seem, to study the causes which appear to favor its epidemicity; for it is there that documents show us principally the epidemic tendency: not that it follows that epidemics at these points are more cruel than elsewhere, — the contrary is rather the rule, without doubt, on account of a certain acquired immunity, — but because it seems that there the causes of epidemicity should be most evident. It is not so, however; and this for the reason, that in Lower Bengal, for example, for the want of sufficiently precise knowledge as to the places which have the exclusive privilege of endemicity, the endemic and epidemic prevalence of the disease are so confounded with each other, that it is impossible to separate the conditions which favor the one or the other.

Further, the special part which one might attempt to assign to causes of insalubrity, — such as those which result from the alluvium of the Ganges in Bengal, — upon the epidemic prevalence of the disease, would be contradicted by the fact that this development occurs as well under conditions of soil and climate entirely opposite.

All that can be affirmed on this subject is, that, in Bengal, cholera takes on an epidemic form, particularly during the hot season, from April to August, while it is otherwise in the provinces of the northwest; where the greatest epidemics (notably that of 1861) have

raged especially during the months of July and August, and terminated at the beginning of winter. At Bombay, the case is nearly the same as at Calcutta; that is, the epidemics of cholera rage there principally from April to September.

In the Presidency of Madras, where the seasons are less strongly characterized, it is also at the time of the greatest heat that cholera shows itself epidemically with the greatest intensity.

Finally, the great manifestation of cholera in 1817, — which, we would say in passing, did not have its point of departure from Jessore, but had only its principal focus there, as appears from official documents of the time, which show, that many weeks before it broke out at Jessore, the disease was already committing its ravages at two points very distant from that city, the one Chittigond, on the gulf, fifty leagues to the east, and the other, Patna, on the Ganges, a hundred leagues to the northwest of Jessore, — that great outbreak, we say, of which the cholera of our days is the uninterrupted succession, commenced also with the hot season. So that it is impossible not to perceive, that in Bengal, as in India in general, and everywhere else for that matter, the hot season exercises an influence favorable to the epidemic development of cholera. But it is only an auxiliary circumstance, subject to numerous exceptions; we do not see it, even in India, a *sine qua non* of the development of an epidemic; with much greater reason, this circumstance, considered by itself, should not be regarded as the actual cause of epidemicity.

The Commission does not think it necessary to discuss the agency of a host of conditions more or less favorable to the epidemic development of cholera in India; nothing would result from it not equally applicable to many other diseases; and it would be a matter of secondary interest. The Commission hastens to consider the circumstances of which the special action is far more evident; we mean the great *agglomerations and migrations* of men, and particularly the *pilgrimages* which are made at special seasons in many parts of India.

We have seen above that cholera rages with more or less intensity under the epidemic form, almost every year, in those places where the Hindoo pilgrims assemble. Of those places, of which

some are at the same time places of worship and traffic, among the most sacred are Hurdwar, Ramdeo, Multra, Ajudhia, Allahabad, Mirsapore and Gya in the northern parts of India; Balassore, Mahadeo and Juggernath, near to Poorie, more in the centre; Trivellore, Tripetty, Conjeveram, Seringham and Ramiseram in the Presidency of Madras; Dakoor, Kodunpore, Sholapore, Sungum, in the Presidency of Bombay.

It will suffice, to give an idea of these agglomerations, to say something of Hurdwar, Juggernath and Conjeveram, which are the most important among these places of pilgrimage, of which the number is very considerable.

Hurdwar is in the northern part of Hindostan, on the Ganges, at the place where this river quits the mountains to begin its course through the plains. A fair is held there every year at the full moon of April, and every twelfth year the pilgrimage to this spot is considered more efficacious than usual; so that at this time the assemblage at this place is enormous. It is said, that in 1783 there were gathered here more than a million of persons, when the cholera broke out and destroyed twenty thousand people in eight days. It is added, that, when the fair was ended, and this crowd dispersed, the epidemic died out without spreading farther, — without even touching the village of Jumalpore, distant only a few leagues. It is not usually so in our day. At present, cholera shows itself at Hurdwar almost every year, at the time of the fair.

Juggernath, on the coast of Orissa, to the northwest of the Gulf of Bengal, is one of the most sacred places. The ceremonies take place there during the months of June and July. The town of Poorie, which is close by, and which at ordinary times counts thirty-five thousand souls, has, during the festival, its population increased to from a hundred to a hundred and fifty thousand persons, and even more, according to some authors.

Cholera breaks out here every year, two or three days after the assembling of the crowd, and does not disappear until after its dispersal at the conclusion of the ceremonies.

Conjeveram is forty-five miles to the south of Madras, and receives every year, during the month of May, at least two hundred thousand pilgrims. The ceremonies last ten days. Dr.

Montgomery, in his interesting narrative (*Medical Times and Gazette*, January, 1866), says that at ordinary times the conditions for health in the town of Conjeveram are not bad, but that this is not the case at the time of the pilgrimage, and that then the cholera breaks out every year. He adds that, in 1864, the government having commenced to adopt hygienic measures (the removal of filth, the establishment of latrines, the separation of domestic animals, abundance of drinking water, etc.), the pilgrimage passed off without cholera. It is true, he remarks, that this year the cholera prevailed very slightly in the middle of the Presidency; but, he adds, in 1865, after the same precautions, Conjeveram remains also free from cholera, although the season has been very unhealthy.

That which occurs in all the places above cited, happens in almost all the holy places. The pilgrims arrive from all parts; and, often after a journey of many hundreds of leagues, made almost always on foot, during the hot season, they arrive exhausted by fatigue and misery. Once in the holy cities, their condition is aggravated still more by the horrible agglomeration, by all the causes of infection resulting from it, — bad nourishment, bad water, debauchery; in a word, by a host of circumstances favorable to the development of cholera among them. Then, finally, when the multitudes disperse, they go scattering cholera everywhere on their journey, and become thus the agents more or less active of the propagation of the epidemic.

In these descriptions, which are the result of observations made especially in these latter years, do we not find, *on a larger scale*, the exact representation of what occurs at Mecca? Here, as at Mecca, cholera does not break out with violence until some days after the gathering of the pilgrims, and it is dispersed and propagated with them in all directions. The pilgrimages of India, then, as at Mecca, must be at the same time the foci of reinforcement and the disseminating foci of the disease. Nevertheless, there is wanting to the complete resemblance an important feature, or rather the existence of this feature which seems to be wanting has not yet been demonstrated in an incontestable manner. At Mecca, it appears to be established, that cholera is always imported.

Is it the same with the agglomerations of India? Or does the disease develop itself spontaneously without previous importation?

It is to be remarked, that the localities in question are not considered as foci of endemic cholera; that cholera dies out after the departure of the pilgrims; and that it does not reappear, more or less periodically, except during a pilgrimage. It is then very important to ascertain conclusively, by carefully conducted inquiries, whether or not cholera is always imported into the places of pilgrimage by individuals coming from endemic or epidemic foci. In the mean time, if we may judge from analogy, *the probability is, that in India, as everywhere else outside of the endemic foci, the importation of cholera is a necessary condition of its epidemic development.*

However this may be, from what goes before it is impossible not to recognize, that, in India, pilgrimages have a capital influence on the development and the propagation of cholera epidemics. They follow, but in a much less degree, the movements of troops, as has been observed principally in the Presidency of Madras.

Finally, if to these causes is added the constantly increasing facility of rapid communications, either by railroad or steamboats, is there not reason to fear an increasing frequency, and a more and more rapid extension, of the epidemics of cholera in India, and, in consequence, an equally increasing danger of importation into Europe?

The Commission, therefore, believes itself authorized to answer, that *pilgrimages are, in India, the most powerful of all the causes which concur in the development and the propagation of epidemics of cholera.*

Adopted unanimously.

SECOND GROUP OF QUESTIONS.

Transmissibility and Propagation of Cholera.

In this group, as in the preceding, the Commission has not thought itself obliged to confine itself rigorously either to the order or the letter of the questions in the programme. It is bent upon presenting the facts, as well as the deductions flowing from them, in their natural order.

VIII.

Is the transmissibility of cholera proved to-day by facts which do not admit of any other interpretation?

The transmissibility of cholera is to-day a fact so established in science that it has seemed, to some persons, almost superfluous to demonstrate it; but an immense majority of the Commission have thought that this demonstration would not be useless, either for the purpose of convincing some incredulous persons who have yet need of light, or at least to show that the Commission has not pronounced upon the question hastily.

The transmissibility of cholera is proved, first, by the course of epidemics generally considered; second, by well-established facts of propagation, after importation of the disease; third, by the evolution of epidemics in infected localities; fourth, finally, by the efficacy of certain preventive measures.

1. Proofs drawn from the course of epidemics considered in general.

Since the first epidemic of cholera which appeared in Europe, it has always been remarked, that the disease followed by preference the great routes of communication, navigable rivers, frequented routes, and masses of men in movement. The latter epidemics have only confirmed this observation. It has been possible to follow them all, like the first, in some way, step by step, from their point of origin in India up to their arrival at a given point; and, whether, as in the first two invasions of Europe, the disease has followed the land route, or, as in 1865, it has more particularly followed the track of navigation, the law of its propagation remains the same, — that is to say, the extension of the disease has always taken place in the direction of the human currents departing from the spot where it reigned.

Cholera has never shown a preference in its course, as some have believed, for a fatal direction from east to west, but, on the contrary, it has radiated and radiates from India in all directions, to the south as well as to the north, to the east as well as to the west, spreading everywhere, by reason of the facility and multiplicity of communications. Those who have believed the contrary have not

studied the facts, and they have reasoned like the Chinese, who pretend that cholera always moves from the west to the east.

Never has this law of propagation by purely human currents been better illustrated than by the epidemic of 1865.

Imported by pilgrims returning from India, it broke out at Mecca during the feast of Courban-Bairam, in May; it followed the pilgrims in their return by way of Egypt, and appeared at Alexandria in the first days of June, after the arrival of the hadjis, by the railroad of Suez. Now, from Alexandria, rapidly converted into a vast focus of emission, does the cholera take a single direction? No. It radiates in all the directions followed by steam navigation. Soon it appears, almost simultaneously, at Beyrout, at Smyrna, at Constantinople, at Malta, at Ancona, at Marseilles, — that is, in those places to which the principal currents from Alexandria have led; while, up to this moment, it has not shown itself at any other points of the coast. These ports, once invaded, became, in their turn, new foci of emission, from which the disease extended in different directions, but always along the line of the great routes of travel; and it was then that railroads became, as we shall see hereafter, a means of rapid importation to great distances.

This is not all. While the epidemic radiated thus on the side of Europe, on the other side it returned, so to speak, upon its steps, with the Javanese and Persian pilgrims returning from Mecca. The return of the Persians was marked by an explosion of the disease at Bassora, at the bottom of the Persian Gulf; and there are reasons for believing, from information communicated by our honorable colleague Dr. Van-Geuns, that it was re-imported into Samarang, Java, by the Javanese pilgrims.

This course of the epidemic in 1865 is so striking, that it has dissipated many doubts; but, since analogous facts were not wanting in previous epidemics, and to complete the demonstration that the direction of cholera epidemics is subordinate to that of human currents, let us recall the memorable fact of the importation, in 1854, of the cholera into the East and into the French army by ships from Marseilles loaded with troops from localities in the immediate neighborhood of which the disease prevailed.

In support of this same law may be cited the constant fact, that every time that the cholera has shown itself on an island, or has been introduced into America, it has always been at some maritime town, ordinarily a principal seaport, and not in the interior of the country, that it has first broken out; as in 1832 at Quebec, and in 1848 at New York and at New Orleans. It is then a feature common to all the epidemics of cholera, observed down to our time, to have constantly followed man in his migrations from an infected to an uninfected place.

This common feature, which shows us the source of cholera attached, so to speak, always to man and never moving without him, is still farther corroborated by this circumstance, very worthy of attention, that the rapidity of cholera epidemics in coming from India to us has been increasing with the increase of intercourse, and, above all, with the greater celerity of the means of transport. It will be sufficient, to prove this, to compare the progress of the first two epidemics which came to us by land, moving with unequal speed, and often delayed by the difficulties of the route, with the prodigious rapidity — *nevertheless not exceeding that of the means of transport employed* — of the course of the epidemic of 1865, which, starting from India at the end of winter, or, if you prefer, leaving Mecca at the end of the Month of May, was able to reach America during the month of October, after having traversed France, and without taking into the account places that it had touched in different directions, in England, in the heart of Germany and in Russia, — thus passing over, in its longest track from India to America, half of the circumference of the earth, in the space of nine months, and arriving from the holy places of Islamism, at Paris, in three months and a half.

Do not all these facts demonstrate to the last point of evidence, that cholera is propagated by man, and with a rapidity so much the greater as his own migrations are active and become more rapid ? The Commission does not hesitate to answer in the affirmative.

Adopted unanimously.

Second. Proofs, drawn from facts, showing the propagation *of cholera by importation.*

If from this first order of proofs, very convincing to any mind

not preoccupied, we pass to the investigation of facts which establish, in an incontestable manner, the transmission of the disease by the arrivals from an infected locality at a place previously healthy, we are only embarrassed by the multitude of facts. We have to deal, indeed, with facts which cannot bear any other reasonable interpretation. The Commission will content itself with a few examples of this kind; for the question for the moment is only to prove that cholera may be transmitted by importation.

These incontestable facts should not be sought for in the great populous centres of the European continent, where the relations between individuals and the movements to and fro are so multiplied and intricate that it is almost impossible to seize upon the rigorous train of circumstances: the conclusive facts are specially furnished by small localities and by seaports where arrivals are easily controlled. In this regard, the actual epidemic is fertile in proofs.

But, before coming to recent facts, the Commission calls to mind, that already Dr. Jukes, in a report to the government of Bombay, has said, with regard to the epidemic of 1817, in India: "It is impossible not to see that the disease has followed the great routes from the Deccan to Panwell; and I do not know a single village in the Concan which has been affected with the disease, without having been visited by people coming from one of the infected places." *Report of the Medical Board of Bombay*, 1819.

The Commission thinks it its duty also to recall the case, so well-known and so characteristic, of the English frigate "Topaz," which, in 1819, coming from Calcutta, imported the cholera into the island of Mauritius. It will limit itself to mentioning in addition, the importation, in 1832, of cholera from England into Holland at Schevening, a small village half a league from La Haye, by a boatman (*Kiehl*, 1865); that at Quebec, during the same year by emigrants from England; the importation, in 1832, at Oporto, by a ship loaded with troops, coming from Ostend, which had touched at England (*Gomez*); fourteen very conclusive cases communicated by Dr. Pélikan, connected with the epidemic of 1847 in Russia; another case of manifest importation at Sebastopol, in 1848, by a ship from Nicolaiew (*Pélikan*); in the same year, importations into

New York and New Orleans by ships loaded with emigrants from Havre; the importation, so clearly established, in 1849, into Nogent-le-Rotrou by nurses and their nurslings from Paris (*Brochart, Memoire presented to the Academy of Medicine, April* 13, 1850), and, in 1853, into the *arrondissement* of Montargis, under similar circumstances, by Dr. Huette (*Archives Génerales de Médecine*); in 1854, the importation, already mentioned above, of cholera into the East, by ships from Marseilles, loaded with troops; in 1853, the importation into Vigo, by a ship coming from Havana; and, in 1865, that into the island of Fogo, in the archipelago of Cape de Verde, by a Sardinian vessel from Savone, loaded with emigrants for Montevideo; the following year, that at Madeira, by a ship loaded with troops from Lisbon (*Gomez*).

Without dwelling upon these facts already published, or many others of the same kind, and equally belonging to science, the Commission prefers to dwell upon facts less known, or unpublished, relating to the last epidemic. It will begin with the fact of the importation into Constantinople.

Importation into Constantinople.

The state of the public health in this city presented nothing which could lead one to anticipate the appearance of an epidemic of cholera, when, on the 28th of June, 1865, there arrived from Alexandria, where the cholera was raging, the frigate "Moukbiri-Sourour." This ship, having been more than five days on the passage, was, on the declaration of the physician that there was no case of suspected disease on board, permitted at once to come to anchor, in accordance with the rules of the port. This statement was false. The evening of the same day, there disembarked from the same ship twelve sick, of whom one was affected with confirmed cholera, and died in the night; and eleven, who only had symptoms of cholerine. On the next day it became known, that, since leaving Alexandria, cases of diarrhœa had been observed on board, and that, in the passage from the Dardanelles to Constantinople, the bodies of two men, who had died of cholera, had been thrown into the sea. The 30th of June, nine other cases, of which two were cases of well-marked cholera, were disembarked from the same ves-

sel, which, after she had replenished her crew, was sent to quarantine near the mouth of the Black Sea.

The sick were taken to the Marine Hospital, near the Arsenal; but, as the way from the landing to the hospital was obstructed with building materials, it was necessary to pass through a barrack occupied by the military workmen of the arsenal. This circumstance should be noted; for the first indigenous cases of cholera occurred among these workmen, and on board a corvette moored close by the barrack.

The 3d of July, one of these military workmen was received into the hospital with a choleriform diarrhœa, and on the 5th he presented all the symptoms of cholera. The same day, a new case occurred among the workmen, and on board the corvette mentioned above. The barrack was then evacuated, and the workmen were placed in tents on the heights of Ok-Meidan. Nevertheless, the cholera continued to rage among them, and on board the ships moored near the arsenal; in addition, it affected, on one side, the guard of the interior of this establishment, and, on the other, the masons who were working on the building of the Minister of Marine, situated very near the barrack of the military workmen. The 8th of July, two cases, followed by death, occurred outside of the arsenal, — a boatman and a fisherman. On the 10th of July, the epidemic began to invade the quarter of Cassim-Pacha, near the arsenal, and occupied by the workmen engaged in the building above mentioned. From there it extended, as will be seen hereafter, to the rest of the city.

This narrative, of which the principal details have been collected and reported by Dr. Mühlig (*Gazette Medicale d'Orient*, August, 1865) — a narrative the exactness of which, in every point, is incontestable — offers an indubitable example of the transmission of cholera by importation, which, although well restricted, was followed by an epidemic of great severity. It does not seem possible to admit a doubt here of the relation of cause to effect between the imported disease and that developed consecutively in the very place where the importation occurred.

The following is a case of importation by land to a considerable distance from the infected place, and without the intermediate

localities having been contaminated, — an importation giving rise to a very deadly epidemic:

The Importation into Borchi.

On the 7th of August, 1865, a number of German families, coming from Prussia, arrived at the village of Borchi, in the district of Balta, in Russia, to be employed on the railroad. On their way, they had stopped a day (the 4th) at Galatz, where the cholera was raging; and had passed through Odessa on the 5th. All these Germans appeared, on their arrival at Borchi, to be in good health; a single child only, belonging to a family of the name of Jans, affected with diarrhœa, died on the 10th of August. From that day, cholera began to show itself, and to rage violently among the inhabitants of the village, and among the newly-arrived Germans. The mother of the infant Jans fell sick on the 18th of August, and died on the 20th. Soon after, two other children of this woman died. Of eight of the Germans attacked, only one recovered. From Borchi, the disease was propagated to the village of Gavinossa, and to a great distance. — *Extract from an official document. Journal de St. Petersburg, No.* 283, 1865.

The Commission thinks it unnecessary to dwell upon the inevitable deduction from this case, of which the authenticity is beyond dispute.

It passes to another example still more interesting, inasmuch as it proves that a single case of cholera, imported from a very great distance by railroad, may give rise to an epidemic.

Importation into Altenburg.

At the end of August, 1865, cholera broke out suddenly at Altenburg, in Saxony, in the centre of Germany. The first case occurred in the person of Mrs. E., who had left Odessa on the 16th of August, and had arrived at Altenburg on the 24th, without having stopped on the way. This lady had with her her child, aged twenty-one months, suffering from diarrhœa. She stopped with her brother, Kunstgasse, No. 678. On the 27th of August, Dr. Geinitz was called to the child, whose diarrhœa had become very severe. The

mother, who was perfectly well, stated that, at the time of her departure from Odessa, there was no epidemic disease in that city (we know that six cases of cholera, imported from Constantinople, were already in the lazaretto, and that on the day following her departure the disease appeared in the city), and that, on embarking to ascend the Danube, everybody seemed well on board, although the steamer passed several localities where the cholera was raging (it is not stated in this narrative whether, during the passage, there was any communication with these localities). However this may be, three days after her arrival at Altenburg, on the 27th of August, the same day on which Dr. Geinitz had visited her infant, Mrs. E. fell sick, and on the next day Dr. Geinitz recognized all the symptoms of Asiatic cholera. She died on the 29th. The same day, in the same house, the sister-in-law of Mrs. E. was attacked, and died on the 30th. The child died on the 31st from exhaustion, the report says. From this house the cholera spread throughout the town and its environs. The family of a workman who died on the 13th of September, at Altenburg, carried the disease to Werdan. The house occupied by this family was the point of departure of an epidemic which destroyed two per cent of the inhabitants of the town. — *Pettenkofer.*

Here is a case, which, if it had not been the subject of attentive inquiry by distinguished physicians, could not fail to have been quoted as an example of the spontaneous development of cholera in the middle of Germany; but the great authority of Pettenkofer, who has made a special study of this epidemic, leaves no room for doubt. Whatever may have been the spot where Mrs. E. and her child contracted cholera, it is still true that they had passed localities where the disease existed, and that, having arrived at Altenburg, they became the starting point for an epidemic. Certainly cases of this kind are not rare, and, with the increasing rapidity of communications, it is probable that they will become more and more frequent: but what is really rare is, first, that they present themselves with such perfect distinctness; and next that pains have been taken, where it has been possible, to determine by inquiry, — in cases apparently of an opposite character, — whether the importation has in reality occurred or not.

In the case in question, was it the child affected with choleraic diarrhœa which communicated the disease to its mother, as one is tempted to admit, on account of the perfect state of health of Mrs. E. on her arrival at Altenberg? Or did she take the germ of the disease under the same circumstances as her child? We cannot say. At the same time, it cannot escape the observation of any one that, under the first supposition, it would be proved that a single case of cholerine imported into a locality may there be the point of departure of an epidemic of cholera. But the Commission does not find this fact sufficiently proved to draw from it this conclusion.

The Commission will finish these citations on the subject of cholera transmitted by importations, with a case quite as characteristic as the preceding, although more limited in its consequences.

Importation into Thoydon-Bois, in England.

In 1865, cholera made its appearance in England, but once and to a very limited extent; but not without carrying with it the proof of its transmissibility. In September, it appeared at Southampton.

At that time, Mr. and Mrs. Groombridge, of the village of Thoydon-Bois, two miles from Epping, county of Essex, were staying at Weymouth, for the benefit of the health of Mr. Groombridge, who was suffering from an intestinal affection. On the 25th of September, they returned to Thoydon, after passing by Southampton, where the cholera existed. Already, on the journey home, Madam Groombridge was feeling indisposed. On the 26th of September, the Drs. McNab were called to her, and, except for a slight diarrhœa, they found nothing alarming in her condition. On the 28th, the symptoms of Asiatic cholera appeared, in consequence of which she died on the 9th of October. On the 30th of September, her daughter Emily, aged seven, was taken sick and died in the course of nine hours; the same day a domestic of the family was taken, but recovered.

The Drs. McNab, during all this time, had been very assiduous in their attentions to the sick. On the 2d of October, Dr. McNab, the elder, was taken with cholera, and died on the 3d.

Two other attacks occurred on the 2d, in the house of Mr. Groombridge; their daughter Kate, and a female servant, were attacked and recovered. On the 6th of October, Mr. Groombridge himself, one of his laborers named Riley, the mother of Madam Groombridge, and Mr. Charles Groombridge, were taken, and all died, with one exception.

The man Riley, whom they had conveyed to his own house, died there on the 7th; a woman, named Saville, who had taken care of him and had laid out the body, was taken on the 7th and died the next day. The disease extended no farther. Nevertheless, two other cases, connected with the preceding, took place afterwards at Coppice Row, situated half a league from the Groombridge house, in the family of a laborer named Haggar. One of these cases, that of Henry Haggar, terminated in death on the 2d of November, in the space of twenty-two hours. The wife of Haggar was the daughter of the woman Saville, mentioned above, whom she had nursed during her sickness. She had changed her clothes before returning home, but she had washed them afterwards in her own house. — *Medical Times and Gazette*, 1865.

It would be difficult to find a more conclusive case of cholera contracted in an infected locality (Southampton), and imported into a healthy place (the Groombridge house at Thoydon), where the disease was communicated exclusively to persons who had had relations more or less direct with the sick. We cannot invoke here an epidemic influence resting upon the locality; for the circumscribed epidemic, so to speak, in a single house, manifested itself nowhere else in the vicinity. The reason why the disease did not extend, we know not, as in many other cases; but what this fact establishes, without any other reasonable interpretation, is the transmission of the disease by communication with the sick.

Opposed to facts of this kind are the cases where, after communication with an infected place, the first attacks of cholera are not manifested in the persons coming from that place, but among the inhabitants of a locality up to that time exempt, and even where it is impossible to trace any connection between the two sets of cases. But those who make this objection go upon a principle contradicted by observation, as will be demonstrated hereafter, — a

dangerous principle, which consists in not admitting the possibility of the importation and propagation of cholera, except by individuals themselves affected with the confirmed disease.

The Commission contents itself, for the moment, with this simple remark, not wishing to anticipate that which will be the object of an ulterior examination.

Third. Proofs drawn from the progress of epidemics of cholera in infected localities.

A third order of proofs of the transmissibility of cholera is furnished by the mode of progression of epidemics, whether in different localities of a country, or in a single locality, from one quarter to another.

This order of proofs in reality does not differ from the preceding; it is the verification of transmissibility by the way in which the disease, once declared, diffuses itself.

It is established by facts demonstrated by observation, that the more dense the population of a place where cholera appears, the more intimate their relations, the more rapid the means of intercommunication, the more rapid also are the diffusion and the extension of the disease; although we do not mean to say that it is necessarily the more violent. It is the confirmation of the law of the movement of epidemics of cholera in general. It is clear, that, in order to trace the connection of facts in the point of view which we are now considering, it is not to populous localities, where the relations are inextricable, that we must go to study them; but only among sparse populations, forming small communities, having, with each other, means of communication which can be easily traced. Nevertheless, Constantinople, which may be considered in its *ensemble* as a vast assemblage of distinct localities, separated by natural obstacles, is an exception in this respect; so that the extension of the last epidemic could be there followed with strict exactness up to the moment of its general diffusion.

It may be laid down also as a rule, resulting from observation, that an epidemic of cholera in its commencement does not appear simultaneously in many localities in the same district, but begins with one of these localities. It is the same generally in large cities, where the epidemic does not manifest itself all at once in

many points at a time, but breaks out ordinarily by a series of cases in a single quarter, sometimes in a single house, before showing itself in other parts of the city. Nevertheless this rule is not without exception, for the reason that, in a great city, where the influx is considerable, importation may take place simultaneously at many points.

Progress of the Epidemic at Constantinople.

At Constantinople, as we have already seen, cholera broke out all at once in the arsenal, where it had been brought in by the sick disembarked from the "Moukbiri-Sourour" on the 28th of June; from the arsenal it spread to the adjoining quarter, Cassim-Pacha; then a few cases appeared in different parts of the city, and, for the most part, among persons who had fled from the quarter first attacked.

Up to the 16th of July, the number of deaths by cholera in the whole city (except those in the Marine Hospital) was one hundred and thirty, when suddenly it was announced that the disease had broken out with violence at Ieni-Keui, a village situated on the Bosphorus, twelve or fifteen kilometres from the quarter where the epidemic was raging. Was this one of its caprices, one of those inexplicable leaps which have formerly been attributed to a cholera epidemic, and which have been made use of as an argument to prove the epidemicity, pure and simple, of the disease? By no means. It is proved that the first case of cholera at Ieni-Keui occurred on the 11th of July, in a Turkish café, in the person of a workman coming from Cassim-Pacha; that on the next day many individuals who frequented this café fell sick, and that two of them died; that on the following days the disease increased in this quarter up to the 16th, when, in consequence of many deaths occurring in families of importance, a great panic seized the whole population of the village, which almost to a man took flight in different directions. Mussulmans, Greeks, Armenians and Jews fled for refuge to other villages and to quarters of the town as yet unaffected, where they carried the disease. The Jews, especially, who had been the most tried, and who, in their haste, carried with them

their soiled effects and their dead, became the principal propagating agents of the disease. At Kouskoundjouk, at Has Keui, and at Balata, the epidemic broke out immediately after the arrival of these fugitives; the statements of the sanitary administration and those of the special Commission leave no doubt on this point. From this moment dates the general spread of the epidemic; after which, except in certain villages, it becomes more difficult to trace its course. These facts are given with much detail in many articles in the *Gazette Médicale d'Orient* (1865–1866), and in a recent work by Dr. Mongéri. (*Etude Sur l'Epidémie de Choléra à Constantinople en* 1865.)

If the Commission did not deem it superfluous to insist on this order of proofs, and were not fearful of prolonging unnecessarily this report, it would cite a multitude of authentic narratives in proof of the successive development of cholera epidemics by successive communications between points where the disease prevailed and others afterwards affected, wherever this inquiry has been conducted with rigor. It might cite, among others, the development of the last epidemic at Odessa, in the month of August last, having for its point of departure people employed in the neighbourhood of the lazaretto, where there were six cholera patients, who had come from Constantinople, these people propagating the disease in their quarter, and from there to the rest of the city.

It might also relate the story of the course of the epidemic in Podolia, the origin of which was its importation into Borchi by the German families, of which we have already given an account; but the Commission, after all which has been said, believes it useless to pursue the subject farther.

Fourth. Proofs drawn from the efficacy of certain preventive measures.

A last order of facts which militate in an indirect manner in favor of transmissibility relates to the efficacy of certain preventive measures. The Commission refers to sequestration, rigorous sequestration, and, above all, to the interruption of maritime communications with infected places.

In 1831, the Imperial Court of Russia, in all ten thousand per-

sons, was isolated at Peterhoff and Tsarskoje-Sélo, and no attack of cholera was observed.—*Report of Drs. Barry and Russell*, 1831.

During the epidemic of 1865, at Constantinople, the pupils of the Military School, to the number of five hundred, were kept by themselves in their establishment, and cholera did not penetrate there, although it raged in the neighborhood. It would be easy to cite many analogous facts in all epidemics.

Greece had escaped the preceding epidemics of cholera, while a rigorous quarantine had been applied to all arrivals from infected places. In 1854, cholera was imported into the Piræus without any obstacle, and a cruel epidemic broke out. In 1865, Greece maintained a rigorous system of isolation, and was preserved, although the disease raged in its neighborhood.

Sicily, which, in 1854, had, like Greece, and for the same reason, been rudely tried, has done more than that country during the last epidemic: it broke off all communication with infected localities, and in spite of the continual passage at a short distance, through the Strait of Messina, of ships coming from countries where cholera was raging, Messina and all Sicily have been entirely free from it.

To these facts it is objected, that many other places visited without restriction by people coming from cholera-infected places have been in the same way spared, while others have not been secured, either by cordons nor very severe quarantines; so that coincidence between the employment of preventive measures and preservation is far from being a constant fact.

To these objections, the Commission answers at once, that it does not pretend, and no one claims, that cholera imported is always transmitted. Transmission necessitates accessory circumstances, which happily are by no means always met with. Were it not for this, the ravages of cholera would soon bring about the extinction of the human race.

And as for the frequent inefficacy of measures of quarantine, the Commission thinks, that before coming to such a conclusion, it would be well to examine whether the measures spoken of have been well calculated, either by their nature or in their application, to accomplish the end proposed; and whether the measures often adopted for precautionary purposes are not the very means calcu-

lated for the propagation of the disease, — sanitary cordons, for example. In such a case, it is important not to be satisfied with words, — measures are measures. It is necessary to examine, before pronouncing in a general way, whether such or such a method, which might be supposed efficacious, and which has been shown to be powerless, was in reality well calculated to prevent the evil against which it was employed.

The Commission considers therefore as of little weight the arguments mentioned above. At all events, it is clear, that, from the inefficacy of quarantines, nothing should be concluded against the transmissibility of cholera.

The Commission, with unanimity, concludes, that *the transmissibility of Asiatic cholera is an incontestable truth, proved by facts which do not admit of any other interpretation.*

Adopted unanimously.

IX.

Are there conclusive facts which force us to admit that cholera can propagate itself to a distance by certain states of the atmosphere, by winds, or by any other similar change or modification of the surrounding medium?

From the moment that it is demonstrated, that, outside of India, or, at least, in the countries previously enumerated, epidemic cholera is never developed spontaneously, and that it has always come by invading epidemics, there is no need of examining, whether, in our countries, certain states of the atmosphere or the soil, certain hygienic conditions, can give rise to epidemic cholera; we need only ask, if, independently of importation by man, cholera can be transported to a great distance by an atmosphere contaminated or modified, — in other words, whether the atmosphere can serve as a vehicle for the principle of the disease, and propagate it at a great distance.

The question thus presented is of great importance, since, if it were answered in the affirmative, it would follow that quarantine restrictions would have but a very questionable efficacy. The Commission has therefore studied it with great care.

To prove that cholera may be propagated by the atmosphere

beyond a certain distance, we need, at least, one conclusive fact; that is to say, one which establishes *the passage of the disease from an infected place to a healthy one without any possible previous communication.* Now this fact does not exist in science; and the Commission has been able to satisfy itself of the frivolousness of all the assertions which have been made of this kind. When people have spoken of deserts crossed, of seas traversed, of powerless *cordons sanitaires*, they have not taken the pains to ascertain whether these assertions were justified by facts; they have ignored the fact that observation has demonstrated, that deserts have always been the most effectual barriers against the propagation of cholera, for the very simple reason that the means of communication across them are more difficult than anywhere else; they have lost sight of the circumstance that, if the sea is an obstacle, it permits, on the other hand, communications, both easy and dangerous; and they have not even asked whether *cordons sanitaires*, such as have been organized in populous places, have not been precisely the most effectual means of propagation.

Undoubtedly, it has not always been possible to demonstrate the previous communication between an infected place and one subsequently infected, and this for the reasons already given; but wherever the inquiry has been made with care, and where the conditions of the locality admitted of a verification, this previous communication has been established.

No conclusive fact then shows, that cholera has been transported to a distance, from one place to another, by the simple intervention of the atmosphere. And at the same time it is incontestable, as will be stated hereafter, that the surrounding air is the principal, if not the only vehicle, of the cholera principle; a circumstance to be noted, but which, no more for cholera than typhus, implies the possibility of transportation to a great distance.

In the absence of any conclusive fact to prove it, we might still believe in transportation by the atmosphere alone, if it could be established that the passage of cholera from one point to another has been, in a single instance, more rapid than the means of communication employed by man. Now, *it is a law, up to the*

present time without exception, that cholera has never advanced more rapidly than man in his migrations.

If we carefully consider the last epidemic, and seek throughout the whole basin of the Mediterranean for a single point where cholera has broken out before communication with a cholera focus, it will not be found. Has cholera progressed gradually in the direction of certain atmospheric currents? No: *it has radiated, and has appeared only where there have been arrivals from infected places, and nowhere else.* Would it have been the same, if cholera travelled in the atmosphere? No: we should have seen it manifesting itself at points where there had been no communication with infected places.

The Commission answers, then, that *no fact has yet proved that cholera can propagate itself to a distance, by the atmosphere alone, whatever may be its condition; and that, besides, it is a law without exception, that never has an epidemic of cholera extended from one point to another in a shorter time than was necessary for man to carry it.*

Adopted unanimously.

X.

How is the Importation of Cholera Effected, and what are the Agents of its Transmission.

According to all facts as yet known, in order that cholera may break out and be propagated in any locality, two conditions are necessary; an arrival from an infected district, and circumstances favoring its transmission. We will confine ourselves for the moment to the first of these conditions. An arrival of cholera is a complex affair. It comprehends man and all that belongs directly to him; next his baggage, his effects, his merchandise, his animals, the ship which brings him— in fact everything which may accompany him. Cholera is undoubtedly transmissible by such means; but is every such arrival liable to transmit the disease? Is every arrival of this kind equally effective in spreading it?

It has been believed for a long time, so far as maritime communications are concerned, that the intervention of a few days between

the time of departure and arrival without any appearance of cholera, was a sufficient guaranty against the importation of the disease. Now experience has shown that it is not so at all; and certain well-established facts tend to prove, that even a long voyage without any appreciable symptoms is no security against danger. On the other hand, it is certain that regular packets, which have run in the India service for a great number of years, have never introduced the cholera into Suez; so that we may say, without farther specification for the moment, that if every arrival from a country infected with cholera is not liable to propagate the disease, it is none the less prudent, in the present state of things, to consider every such arrival as suspected. A more detailed examination will show this.

Adopted unanimously.

XI.

Under what conditions does man import cholera?

Man infected with cholera is the principal importing agent of the disease: this is undoubted. There are abundant facts to demonstrate it. In a very great number of epidemics, where their origin has been made out, at the point of departure there have been found one or more cholera patients who have come from without. It would be tedious to add other facts to those which we have already cited. What is equally proved by observation is, that the arrival of a great number of cholera patients in a healthy locality is not necessary to develop there a great epidemic. Most frequently it is a few cases, as at the Piræus, at Varna in 1864, at Constantinople during the last year, as at many other places too numerous to mention; sometimes it is a single case, which — as at Altenburg — has been the point of departure of an epidemic. In this respect, the study of facts demonstrates that there is no proportion between the mass of cholera imported and the intensity of the epidemic which follows; this intensity is in relation with the conditions, more or less favorable, of the community where the disease has penetrated; just as a conflagration is in no proportion to the spark which originates it, but to the combustibility and abundance of inflammable material which it meets with.

Thus, *man affected with cholera is himself the principal agent in propagating this disease, and a single cholera patient may cause the development of an epidemic.*

Adopted unanimously.

XII.

We may go farther. Authors of high authority (Pettenkofer, Hirsch, Griesinger) affirm, and offer facts which tend to prove, that an individual coming from a cholera district, and only affected with a diarrhœa, may import into a healthy locality, and there propagate, cholera. A member of the Commission (Dr. Millingen) has brought forward many analogous facts, taken from a work on cholera published at Copenhagen in 1855, by Dr. Brika. Most of these facts are very interesting, and make the point very probable; but they have not all the clearness necessary for a rigorous demonstration.

A very conclusive case, on account of the details by which it is accompanied, is that reported in the *Gazette Médicale de Paris* (April 28, 1849), by Dr. Alexander, of which the following is a summary. At Hamel, a village twenty-five kilometres from Amiens, there was no sign of cholera, when, on the 4th of April, there arrived at the village, direct from Paris, where the cholera was raging, a soldier named Guilbert, *affected with diarrhœa.* He was received at the paternal mansion where he remained, confined to his bed, for three days; on the fourth he entered the Hotel Dieu of Amiens. On the same day André Guilbert, brother of the soldier, was attacked with the most violent form of cholera and died in twelve hours. This man did not live in his father's house, but he had gone there very often daily, since the arrival of his brother. The wife of André Guilbert was taken, three days after the death of her husband, with cholerine, soon followed by cholera, and died on the 16th of April. The father, Guilbert, who, during the stay of his son, the soldier, at his house, had experienced symptoms of cholerine, was taken with cholera on the 11th, and died on the 15th. Another son aged seventeen years, and a child of four years, the son of André, were affected with cholerine and recovered. The father of André's wife, who had nursed his son-

in-law and his daughter, was taken with genuine cholera and recovered. A child of eleven years, who frequented Guilbert's house, and whose parents had taken care of André and his wife, was attacked with cholera on the 14th and died the next day. As for the soldier who had been the means of introducing the disease into the village, he left the hospital at the end of a few days perfectly cured, and without the appearance there of any disease similar to his own. Dr. Alexander adds, with much reason, that this fact proves, that cholerine is nothing else than cholera, of a less degree of gravity.

This case is, unquestionably, very conclusive; and, added to the cases, more or less analogous, published, it gives great probability to the opinions of authors who admit that the premonitory diarrhœa, or cholerine, may transmit cholera.

In the case of maritime arrivals from infected places, we do not possess an example equally characteristic.

There has already been mentioned, before the whole Commission, the fact of the importation of cholera into the Piræus in 1854, following the disembarkation of four patients affected only with cholerine and who recovered; but there had been already on board the ship two cases of cholera, followed by death, and the crew had communicated with the shore; so that it is impossible to conclude with certainty, that it was these patients who transmitted the disease.

By these considerations, the Commission has been led to conclude, that *certain facts tend to prove that a single individual (with much greater reason many) coming from a contaminated place, and suffering from diarrhœa, may be sufficient to give origin to a cholera epidemic; or, in other words, that the diarrhœa called premonitory is able to transmit cholera.*

As to the question whether individuals coming from a cholera district, and enjoying, on their arrival at an uninfected place, apparently perfect health, are able by their own persons to introduce the disease, the Commission answers that *nothing proves it.*

There are, it is true, examples which show that cholera has broken out in a locality after the arrival of individuals in this condition,—but has it been possible to prove that these individuals were

really in a state of perfect health, — that they had no diarrhœa? No: such a verification, except under particular conditions, is and will always be, in the great number of cases, impossible. And besides, supposing the absence of all premonitory symptoms well established, if cholera breaks out, as we have seen, after such an arrival, should we have the right then to conclude that it is these healthy people who have personally introduced the disease? May they not have brought with them contaminated articles?

Finally, when we enter into the details of the question, we see how difficult it is to determine, in a rigorous manner, the part played in the importation by such or such an element connected with arrivals from an infected locality.

Adopted unanimously.

XIII.

What is the length of the period of incubation?

The question of the importation of cholera by man leads us quite naturally to examine what is the *maximum* duration of the period of incubation of cholera, and up to what point we may take it into account, in a prophylactic point of view.

The length of the period of incubation of cholera — that is to say, the interval of time between the supposed instant when the morbific agent penetrates the organism and the moment when the first symptoms of the disease manifest themselves — this period is generally very short. Observation shows, in fact, that, in an immense majority of instances, a few days are sufficient for the incubation, and that sometimes this period does not exceed a few hours. This general rule is proved beyond a doubt by the first cases which follow the importation of the disease into a healthy locality. We see then, when the disease is transmitted, that a few days (a week at most) hardly elapse between the imported cases and those traceable to them. Later, when the epidemic is fully established, the relation between the cases becoming very difficult to prove, nothing farther can be concluded with certainty with regard to the period of incubation.

Still, the general rule is established beyond a doubt by what ordinarily happens on board ships departing from a cholera focus.

If the disease breaks out, it is, in most cases, during the first days of the voyage; and it is upon this fact, generally admitted, that five days of observation have been fixed upon as quarantine for arrivals from such places. Such, then, is the general rule. But there are exceptional cases, which would lead to the belief that the period of incubation may be prolonged more than twenty days.

These facts, to have a positive value, can only be gathered on shipboard. Now, under these circumstances, it is not very rare to see genuine cholera break out on board as late as six or seven days after departure from the contaminated place. The last epidemic furnishes examples of this.

We have cited the case which occurred in 1848, of that vessel, loaded with emigrants, which sailed from Havre to New York on the 9th of November, and on board which cholera did not appear until the sixteenth day of the voyage. When these emigrants, to the number of three hundred and forty-six — Germans, for the most part — embarked, cholera did not yet exist at Havre; but many of these people came from Germany, where the disease existed. There were among them nineteen cases and seven deaths. It is to be noted, that they communicated the disease to thirteen people on Staten Island, where the quarantine was situated.

At the same time (Nov. 3, 1848), on board another vessel, the "Swanton," which had also sailed from Havre, bound for New Orleans, with two hundred and eighty emigrants, cholera did not break out until the 25th of November, — that is to say, on the twenty-third day of the voyage, and caused thirteen deaths. A certain number of these emigrants came, like those of the other ship, from points in Germany where the cholera was raging. (*Baly. Report on Cholera, etc.*, 1854.)

Here is another fact relating to the last epidemic at Gibraltar. On the 21st of August, 1865, when cholera was raging in the town, a part of the first Battalion of the Ninth Regiment, which, up to that time had remained in good health, received orders to sail for the Cape, and was embarked on board the "Renown," a large new vessel, well ventilated. The following day, the 22d of August, a case of cholera, rapidly fatal, occurred on board. The ship was hauled off into the channel, and as no other case had occurred on

board, she went to sea at the end of thirty hours. All went well until the 5th of September; but on that day, when thirteen days out, cholera broke out on board, and, in the space of fourteen days, carried off nine men, one woman, many children, as well as the surgeon of the ship. (*Extract from an official communication by Mr. Goodeve.*)

These facts, although rare, constitute exceptions that it is important not to neglect. We would remark in the first place, that in all these instances it is a question whether the case is one of confirmed cholera. Now as it is well demonstrated to-day that the premonitory diarrhœa is an effect of the cholera poison, a light degree of the disease itself, and in all probability capable of transmitting cholera, it follows that in a practical point of view this diarrhœa ought to be assimilated to an attack of cholera itself, and should not be reckoned in the period of incubation. In other words, according to the Commission, the appearance of cholera on board ship, as everywhere else, does not begin only with the first unmistakable attack, but with the premonitory diarrhœa, which so often precedes the cholera properly so called, and does not end in it in the great majority of cases.

Who then can say, that in these exceptional cases there had not been, before the outbreak of cholera, these cases of diarrhœa, marking the commencement of the disease?

On the other hand, — admitting that there were not, — from the moment that it is proved (as we shall show hereafter), that effects, baggage coming from a focus of cholera, may be the receptacle of the morbific principle and communicate the disease, is there not reason for asking whether in these exceptional cases the disease has not been contracted on board during the voyage?

These exceptions prove then nothing against the rule which shows that the length of the period of incubation of cholera does not extend beyond a few days; but they prove that the length of a voyage, however great, without the occurrence of an outbreak of cholera, is not a certain guaranty against the importation of the disease.

The *maximum* of the duration of the period of incubation cannot be fixed with certainty, except by cases collected outside of

all possible contamination, that is to say, of individuals who have come from an infected place, and entirely removed from all new exposure; such would be a single traveller in whom cholera did not show itself until a certain time after he had left the infected place, provided he did not carry with him any article capable of serving as a receptacle for the morbific principle; such would be the Altenburg lady, if she had not carried with her her sick child and her clothing; but such above all would be the cases of individuals submitted to strict isolation after having been separated from everything capable of transmitting the disease. In these cases, the longest time intervening between the isolation and the manifestation of cholera symptoms would give, supposing the observation extended to a large number of cases, the *maximum* of the period of incubation.

But the Commission is compelled to acknowledge, that facts collected under such conditions do not exist; for the very simple reason, that up to the present time, — cholera not having been supposed transmissible, except by a patient actually affected with the disease at its height of intensity, without any account having been taken of its transmissibility by contaminated objects, — measures have not generally been instituted of a character to establish these points.

For this reason the Commission, governing itself by a general rule, has adopted the following conclusion: *In almost all cases, the period of incubation, that is to say, the time intervening between the moment when an individual may have contracted the cholera poison and the commencement of the premonitory diarrhœa, or of confirmed cholera, does not exceed a few days; all the facts cited of a longer incubation belong to the class of cases where contamination may possibly have taken place after departure from the infected place.*

Adopted unanimously.

XIV.

Can cholera be imported and transmitted by living animals?

This question must be considered under two different points of view: we may ask whether certain animals may not contract chol-

era, and, consequently, transmit it under the same conditions as man; or whether living animals, not sick, may not, like any contaminated article, serve as a receptacle for the principle of the disease, and so import it?

Some very reliable authors, and especially Griesinger, do not admit a doubt that certain animals may be affected with a morbid condition, having the closest analogy with cholera. They appeal to the epizoötiæ, which, during cholera epidemics, have frequently raged among birds, the bovine species, horses, etc.; and, from this coincidence, together with a certain analogy in the symptoms, they infer the identity of their nature. Further, from some experiments made by Meyer, Thiersch, etc., they have concluded that the dejections of cholera patients may transmit the disease to some animals. But all these facts, we would say at once, are far from being conclusive; and even when we admit the analogy of certain epizoötiæ with cholera, and that the white mice poisoned by Thiersch presented all the symptoms of this disease, we should be far from feeling authorized to admit the identity of these affections, and still less the transmissibility of the disease to man. In reality, no case has occurred up to the present time, which gives any value to this opinion.

It remains to be ascertained whether a living animal, by means of its coat, may serve as a receptacle for the principle of the disease. We can understand how this might be. This is all the Commission can say; and in this respect living animals belong to the category of objects capable of importing cholera. In consequence, and without insisting farther on this point, which will be considered in discussing the measures of prophylaxia, the Commission limits itself to answering in the following manner: *There is no fact known which proves that cholera has been imported by living animals; but it is reasonable, nevertheless, to consider them, in certain cases, as belonging to the class of objects called susceptible.*

Adopted unanimously, except by M. M. Bykow and Lenz.

XV.

Can cholera be imported and transmitted by linen, clothing, and in general by articles in common use?

The Commission is unanimous in admitting that the effects in use by cholera patients, or which have been soiled by their dejections, may transmit cholera; and yet, to furnish a rigorous demonstration by examples of which no other interpretation is possible, they have been able to collect but a very small number. It is not that cases which go to support this opinion are rare; they are, on the contrary, very numerous; but, as almost always happens in such cases, they present themselves surrounded by circumstances which admit of different interpretations. Such are the cases collected in any focus of cholera. Thus the remark generally made, that washerwomen are particularly smitten during epidemics, goes unquestionably to support its transmissibility by soiled linen; but as the disease may possibly have been contracted otherwise, it would be necessary, in order that its probability should be well established, that it should be the result of the comparative study of a great number of cases.

Conclusive cases, then, can only be collected outside of cholera foci. Now, under this condition we find recorded in science cases of transmission by linen coming from an infected place. It was thus that in 1853, at Cessantes, near Vigo, cholera was communicated to two washerwomen who washed the linen from a lazaretto where the disease existed, while yet their village, the town and all the province, were free from the disease. (*Monlau.*)

The first case of cholera observed in the village of Moor-Monkton, six miles from the city of York, occurred on the 28th of December, 1832. At that moment the disease did not exist in the neighborhood, nor at any place nearer than thirty miles. A laborer by the name of John Barnes, aged thirty-nine years, had been suffering for two days with diarrhœa and cramps, when, on the 28th of December, he was taken with all the symptoms of cholera with collapse, and died the next day. The patient had been visited by two respectable physicians, the brothers Hopps; of

whom one, a very experienced practitioner of the city of York, immediately instituted an inquiry to ascertain the probable source of the disease. His first investigations were vain. In the mean time, the wife of John Barnes and two other persons, Metcalfe and Muscroft, who had visited the patient the day before, were taken themselves with cholera; they recovered. In addition, John Foster, Ann Dunn and the widow Breyke, who had all been in communication with the aforesaid patients, were all affected with a severe premonitory indisposition, which was however arrested. While the physicians were seeking in vain to discover the origin of the disease, the mystery was revealed in an unexpected manner by the arrival of a son of the defunct. The young man had been apprenticed as a shoemaker with his uncle, at the town of Leeds. He informed the physicians that his aunt (the sister of his father) had died of cholera fifteen days before, and that, as she had no children, her effects had been sent to John Barnes by the regular conveyance, and without having been washed. John Barnes had opened the trunk in the evening, and the next morning he was taken sick. *J. Simpson, Observations on Asiatic Cholera. London*, 1849.

Dr. Simpson relates in the same work a very curious fact, which tends to prove that a contaminated object, if shut up, has, ten months after, communicated cholera. The case was observed in York, in 1833, by Dr. Brown. A woman, aged sixty-seven years, died of cholera in the month of August, 1832. Ten months afterwards, at the festival of Whitsunday, two nieces of this woman having come to visit their uncle, he opened, for the first time, a drawer which contained, besides some little trinkets, that he offered to his nieces, the cap which his wife had worn at the time of her death. This man was taken with cholera the same evening, and died the next day. Dr. Simpson, not doubting the veracity of Dr. Brown, gives to this fact a special value.

Dr. Simpson adds: "The cases cited above are of incontestable authenticity. They show, evidently, that the disease may be carried from place to place by means of clothing."

There is another remarkable case, taken from the work of Pettenkofer, — *Untersuchungen und Beobachtungen über die Verbreitungsart der Cholera. München*, 1865.

At Lurtheim, a commune of Schleisheim, near Munich, the first cases of cholera occurred in a family of laborers, composed of father, mother, daughter and a kinswoman. Another daughter was at service in Munich. This last sent to her parents food and the old clothes of a family of which some members had died of cholera. The food, already a little changed, was eaten; the clothing was put on. The third day, the 21st of September, 1854, the father and mother were taken with cholera and died. On the 22d their daughter was attacked. On the 25th, the son, who was at service elsewhere, came to the house to attend the funerals. He fell sick in the afternoon, and died in five hours. The daughter, who was at service in Munich, and who had sent the above-mentioned articles, having come home on the 22d to take care of her sister, fell sick the same day, and died also. On the 26th, the kinswoman, who lived with this family, was taken in her turn, and died also. The only survivor of this family was the daughter, attacked on the 22d.

We find also, in the work of Pettenkofer, the very interesting case of the prisoner, who, transferred from the hall of the police at Munich, where many attacks of cholera had taken place, to the prison of Ebrach, as yet free from the disease, introduced the disease there, although he had on his arrival only the diarrhœa. Having entered on the 20th of August, 1854, he was taken with the characteristic symptoms on the 26th, and recovered; but his keeper, who was taken on the next day, died in a few hours. An epidemic followed. The disease broke out on the 28th in that part of the prison reserved for females, and which is completely separated from that assigned to the men. Pettenkofer ascertained, on inquiry, that the first female attacked had been employed on the 25th in washing the soiled linen left off on the 20th by the prisoner above mentioned.

Lebert (*Choléra en Suisse. Frankfort*, 1856) reports the very interesting case of a man who was taken with the cholera at Lugano after the complete cessation of the disease in that town, and after wearing the clothes of a man who had died with cholera, two months, in the same house. Dr. Pappenheim has related, in Casper's Journal (vol. V. 1854), facts which show that people have

contracted cholera, when the disease had entirely ceased, after sleeping in beds which had been used by cholera patients during an epidemic, and which had been set aside and shut up from that time.

If all these facts, which we might multiply, do not give us absolute certainty, they establish at least such a probability, that we cannot refuse them a very great value.

In the case cited above, of the vessel which left Havre, in 1848, loaded with emigrants for America, and on board which the cholera did not break out until the sixteenth day of the voyage, the outbreak of the disease was attributed to the fact, that the emigrants opened their trunks containing contaminated clothing. This is doubtless only a supposition, but one which becomes very probable, when we consider, on the one hand, the interval of time between their departure from Havre, where the cholera was not yet raging (many of these emigrants came from a point in Germany where the disease existed), and the moment of the first attack; and, on the other hand, all the facts which militate in favor of transmission by articles which have been in relation with cholera patients. In interpreting this fact, there are in reality but four suppositions possible: either the spontaneous development of cholera on board the ship, which would be unexampled; or an incubation of at least sixteen days, which would constitute an exception infinitely rare; or a concealment of the first signs of the cholera poisoning, subsequently recovered from; or, finally, a contamination contracted on board, which is most in harmony with what we know of the modes of transmission.

Side by side with the facts which prove that cholera may be transmitted by articles which have been used by cholera patients, there are others which show that the camping-place where an epidemic has existed in India, the ward of a hospital, the chamber, the ship which has had cholera patients on board, may sometimes preserve, for a certain time and under certain conditions, the power of transmitting the disease. It would be tedious to enumerate the facts already well-known which confirm this assertion beyond a doubt.

But, with regard to the well-demonstrated possibility of the trans-

mission of cholera by effects coming from a focus of cholera, and, above all, by those which have been in relation with the sick, it is proper to remark, that, in the generality of cases, such effects, brought from a place where cholera reigns, do not import the disease. If it were otherwise, in the different epidemics which have occurred, and notably in the last one, the spread of the disease in every direction would have been much greater. In fact, when we consider the immense number of travellers who started in 1865 from a cholera focus, and who were scattered, accompanied by their baggage, through all the countries of Europe, and, consequently, the number of places exposed to contamination by the importation of articles in common use, from an infected place, we are forced to acknowledge, that, if transmission by this cause has been effected in certain localities, it does not occur in the immense majority of cases.

There are, then, certain conditions necessary, and happily rare, to enable articles in common use to import and transmit cholera.

These conditions are indicated to us by the very examples where this transmission has occurred; — that is, for its transportation to a short distance, it is necessary that the effects in question have been recently in direct or indirect relation with cholera patients, and above all, have been soiled by their dejections. This is the circumstance that causes washerwomen, and, in general, persons who have had anything to do with the effects of cholera patients, to be the special victims of the disease. Now it is clear, that this occurrence is likely to be rarely met with from the effects of a traveller. Nevertheless, it is not impossible that linen soiled by an individual having only a simple cholera diarrhœa may be shut up in a trunk. What do the facts show, where the transmission has taken place long after the cessation of the epidemic, or at a place far from the point of departure? They show that the contaminated effects have always been shut up, confined, and more or less kept from contact with fresh air. There is not an instance of articles exposed in the open air, beyond a very brief period (which we cannot give with absolute precision for want of exact data), which have transmitted cholera; while there are cases which go to prove, that transmission has taken place by means of effects kept shut up for many months.

It follows from all this, that articles of common use owe their property of transmitting cholera to the fact that they may be impregnated with matters from cholera patients, and, above all, with their alvine dejections, which, we would say in passing, appear to contain most especially the principle of the disease. It follows also that effects thus contaminated lose very rapidly in the open air the property of transmission, but that this is not the case if these things are kept closely shut up.

The open air is then for such contaminated articles a purifying agent; and far from being able to transport intact to great distances, as has been believed, the generating principle of cholera, it destroys it rapidly, as will be demonstrated hereafter.

Having said this, the Commission answers, that *cholera may be transmitted by articles in common use coming from an infected place, and especially by those which have been used by cholera patients; and that it also follows from certain facts, that the disease may be transported to a distance by these same articles when closely excluded from contact with the outer air.*

Adopted unanimously.

XVI.

Can cholera be imported and transmitted by merchandise?

We are unable to cite a single instance proving the transmission of cholera by merchandise imported from a place where this disease prevailed.

Never, for instance, has merchandise, imported from India, by way of Suez or directly into Europe, transmitted cholera. At the same time, this does not prove that the thing is impossible and has not taken place somewhere else, where it was not noticed; especially if we consider, that under the name of merchandise are included many articles eminently liable to be impregnated with the morbific principle, such as rags, hides, etc.

Therefore the Commission, while acknowledging with unanimity the absence of proof of the transmission of cholera by merchandise, admits, by a majority of sixteen votes to six, the possibility of the fact under certain conditions.

The negative votes were MM. Bykow, Goodeve, Lenz, Pélikan, Polak and Van-Geuns.

If now we call to mind all that has been said concerning the agents by which it is admitted, with more or less reason, that cholera may be imported and transmitted, we see, as we stated in the commencement, how difficult it is, in the actual state of our knowledge on the subject, to prove the exact part which belongs to such or such an element in an arrival from an infected place, — to man, to his effects, to his merchandise, to the vessel. All these elements are certainly not equally dangerous, but all may be so independently of each other, for the single reason that they all come from a place infected with cholera.

In consequence, until more fully informed, the Commission believes that it will be wise to consider as suspected, at least under particular and determined conditions, every arrival from a cholera focus.

Adopted unanimously, except by MM. Goodeve, Pélikan and Polak, who abstained from voting.

XVII.

Can the dead bodies of cholera patients import and transmit cholera?

In Europe there is no reason to fear that the dead bodies of cholera patients may transport the disease from one place to another, for the reason that when they are conveyed to a distance, it is with such precautions as to avert all danger; but it is not the same in Asia, where, in obedience to certain religious customs, it is usual in many countries to convey dead bodies to great distances. In this respect, the question is specially interesting with regard to Turkey.

We know, in fact, that every year, at a fixed time, the Persians come in pilgrimage to certain consecrated places in the environs of Bagdad, and that they have the custom of bringing with them a great number of dead bodies in all degrees of decomposition, from bones enclosed in sacks or boxes, to the dead of the day before placed in badly joined coffins. These human remains, which exhale an infectious odor, are brought to receive sepulture near the venerated tombs of the great saints of Islamism. Often enough, too,

these pilgrims bring with them the cholera, which spreads more or less in Bagdad and throughout the province.

The influence of the dead bodies in this case is not doubtful; they create conditions of putridity which contribute to augment the ravages of the disease: this follows from all the information received on the subject. But has their introduction ever given rise to the cholera? Our answer to this question is, that always when cholera has been brought to Bagdad by Persian pilgrims, they had it with them before arriving; so that it is difficult to discriminate the influence of the dead from that of the living. On the other hand, when the pilgrims have arrived free from cholera this disease has never broken out, either among them or in their neighborhood, at the time of the ceremonies of burial of the bodies, however great the infection from the putrid exhalations. All, then, that we can conclude is, that the Persian corpses, considered as putrid remains, do not give rise to cholera; but we are not prepared to say that it is the same with the dead bodies of cholera patients.

The opinion adopted by many physicians who have studied the question of late is, that these corpses of cholera patients are very active agents of transmission. It seems, in fact, very natural — transmissibility being admitted — that it should be so, the body being supposed to contain all the elements capable of reproducing the disease. Nevertheless, it is a matter of observation that physicians who are the most occupied with the pathological anatomy of cholera, who have made hundred of autopsies, have not been more affected with the disease than others. This fact has been observed as well in India as in Europe. May it be for the reason that, in general, autopsies in cholera cases are made on bodies just dead, at the moment when a certain degree of fermentation, which may be necessary for the morbific principle, has not yet taken place? It is possible. But, on the other hand, when we seek for conclusive cases to prove the transmission by dead bodies, we do not find them; or, rather, we find complex cases which do not allow us to affirm that it is the dead body rather than the dejections anterior to death, or other circumstances, which have transmitted the disease.

In truth, there is no rigorous demonstration of the fact; and because a cholera patient or his discharges may transmit cholera, it does not necessarily follow that the dead body of a cholera patient, with all that it contains, retains the same property.

In this doubt, the Commission answers, *although it is not proved by conclusive facts that the dead bodies of cholera patients can transmit cholera, it is prudent to consider them as dangerous.*

Adopted unanimously, except by M. Sawas, who abstained from voting.

On the Influence of Means of Communication.

After having studied and determined, as far as possible, the part of the principal agents, to which we may attribute the importation of cholera, it is proper to examine now the part which belongs to means of communication in the propagation of epidemics.

XVIII.

What influence do the different modes of communication, by land or sea, have upon the propagation of cholera?

The progress of epidemics has shown us that the propagation of cholera is always effected in the direction of human currents, that the more active the communications, the more multiplied in any country, or from one country to another, the greater is the tendency to the spread of the disease; the more rapid the means of transport, the more rapid also may be its extension. We have already cited facts in evidence of these propositions, and we need not here revert to them.

Of all the modes of transportation, the most dangerous, the most fitted to convey the disease, although not the most rapid, is transportation by sea; for the reason, that a ship may contain within her all that constitutes a focus of cholera. She may transport, not only a whole epidemic, but she carries it under conditions of confinement and infection the most favorable for the transmission of the disease. A vessel infected with cholera may, then, be considered, we say, as the most certain means of propagating it, and so much the more to be feared the shorter the voyage she has had.

Communications by railroad, although capable of carrying the disease more rapidly from one point to another, as the last epidemic has shown, are not nearly so certain to propagate an epidemic. Experience has shown it, and reason comprehends it. It is very rare that railroads convey cholera patients, and the population which emigrates by this mode of conveyance from an infected place does not, in general, belong to that class which is particularly liable to be attacked by cholera. These circumstances, in addition to free ventilation and all the other causes of extinction of the cholera principle in such a journey, more than compensate for the danger resulting from the number of travellers.. Let us hasten, however, to add that, next to maritime transportation, railroads are, incontestably, the most active agents for the rapid extension of epidemics; and we must admit that, under certain conditions (that, for example, of the conveyance of troops), they may be the cause of a sure propagation. The development of cholera at Alexandria, after the arrival of pilgrims by the railroad of Suez, furnishes proof of this.

Consequently, the Commission answers, that maritime communications are, by their nature, the most dangerous; that is, it is they which the most surely propagate cholera to a distance; and that next come those by railroads, which in a very short time may carry the disease to a great distance.

Adopted unanimously.

XIX.

What is the influence of deserts upon the propagation of cholera?

Among all the means of communication from one country with another, there is one which specially deserves to fix our attention, because, far from favoring the propagation of cholera, it has never served as a guide to its march: we refer to the communication across great deserts by caravans. An experience which goes back to the first appearance of cholera outside of India has taught, in fact, that a great desert is the best of all obstacles to the propagation of cholera. It has demonstrated that not only has such a space never been crossed at a bound by the disease, but even that a numerous caravan, leaving a point where cholera is raging, gradually gets

rid of it on its march across the desert, and arrives entirely purged of it, provided the journey lasts more than twenty days. The Ottoman Sanitary Administration is in possession of very valuable information on this question. Never has a caravan of pilgrims which left Mecca a prey to cholera (and this circumstance has repeatedly occurred), imported the disease into Damas. Documentary evidence proves that when this caravan has left Mecca with the cholera in the midst of it, the disease has always disappeared after a week or two of travel. The same may be said of the caravan which returns from Mecca to Egypt by way of Suez: it has never imported cholera into Egypt; and it has been proved that if in 1831 this disease was introduced there by pilgrims returning from Mecca, it was introduced by those which came back by sea, and not by the caravan, which did not arrive until later.

The same remark is true of the journey across the deserts which separate Bagdad from Damas and Mecca; and when, in 1823, and later, in 1847, the cholera coming from Persia advanced as far as the north of Syria, it was by ascending the Tigris and the Euphrates, by Diarbekir, Orfa, Biredjik, that it penetrated, and not across the desert. A statement contained in the work, otherwise very valuable, of Verollot, on the course of the cholera, in 1845–6–7, tends to prove an exception to this rule. Verollot supposes that cholera was imported into Mecca, in November 1846, the time of the pilgrimage, by Persians from Kerbela, and this only because in the month of August of that year the cholera was raging in Kerbela; but he forgets, that, from the month of May, the disease existed at Djeddah, and that it is much more rational to admit that from this town it spread to Mecca, where it had attained its full development at the time of the pilgrimage. The supposition of Verollot, then, is not of a nature to weaken a rule established by long experience. We will add, that this same rule has been verified for the deserts of Northern Africa, by our colleague Dr. Dickson, during the epidemics of 1850 and 1855, where the cholera never extended beyond three stations in the desert, and that it has been equally sustained by observations made in the United States of America, by Dr. Byrne. — *Essay on Cholera*, 1855.

It is, then, we may say, a well-demonstrated truth, that a great

space, a desert, where the population is scattered, and has with each other but the most limited relations, is the best of all barriers against the importation of cholera; and that, when the disease penetrates it, it evaporates in some way and rapidly becomes extinct. Thus this free air, this atmosphere across which it has been supposed that the principle of cholera could transport itself to great distances, is, on the contrary, the purifier and destroyer of the principle.

The Commission, keeping to facts established by experience, concludes that great deserts are a very efficacious barrier against the propagation of cholera; and it believes that it is without example, that this disease has been imported into Egypt or Syria across the desert by caravans from Mecca.

Adopted by all the members of the Commission, except MM. Monlau, Pélikan, Polak and Van-Geuns, who refrained from voting.

On the Influence of Crowding.

XX.

What is the influence of the crowding together of human beings on the intensity of cholera epidemics, as well as upon the propagation of the disease? And under what conditions does it exercise its influence?

To solve these questions, it is important to study successively this influence as it presents itself on board ship, in lazarettos, in armies, in fairs, in pilgrimages, and especially that of Mecca. *Per contra*, it is necessary to show the influence of scattering, whether as a means of diminishing the intensity of cholera epidemics, or as a means of propagating them.

But, before passing to the detailed study of these different points, the Commission believes itself authorized to answer, for the present, in a general manner, to the questions proposed, that *every collection of human beings, in the midst of which cholera has been introduced, presents a favorable condition for the rapid extension of the disease, and, if this collection exists under bad hygienic conditions, for the violence of the epidemic among them.*

That, in such a case, the rapidity of extension of the disease is proportionate to the concentration of the agglomerated mass, while the violence of the epidemic is, other things being equal, so much the more decided as the individuals composing the crowd have been little exposed to the choleraic influence, or not at all; that is to say, in other words, that individuals who have already been exposed to the influence of a focus of cholera enjoy a sort of relative and temporary immunity, which counterbalances the bad effects of crowding.

That, finally, in a dense crowd, the more rapid the extension, the more prompt, also, is the cessation of the epidemic; at least, if new arrivals of unaffected persons do not furnish new aliment for the disease, and keep it alive.

Adopted unanimously.

The importance of many of these propositions will escape no one. They affirm this very remarkable fact, that the more concentrated the crowd, the more quickly does cholera propagate itself and there expend its action, without perpetuating itself, whatever may be the unfavorable conditions of the place. Experience shows that, under these conditions, cholera, after having made a more or less considerable number of victims, which never goes beyond a certain proportion, is promptly extinguished in consequence of the immunity which the survivors enjoy. The practical consequence of this fact is, that, when cholera has declared itself in such a community, there is no reason to fear that it will last beyond a certain time, nor that it will make an unlimited number of victims. It remains for us to demonstrate now, that these conclusions, applicable in general to all these agglomerations, are justified by the study of facts, considered under each kind of agglomeration in particular.

XXI.

What is the intensity, and what the tenacity of epidemics of cholera on shipboard?

The conditions of crowding on board ship are, without contradiction, the most favorable for the rapid development and the violence of a cholera epidemic. A confined space, badly ventilated, the impossibility of isolating the sick sufficiently, the infection which results therefrom, make a ship crowded with human beings

the most favorable locality for the development of an epidemic, and apparently of continuing it. It cannot be denied that the greater the crowding, the worse the sanitary conditions on board, the more also the probability of a violent epidemic is to be feared. Experience on this point agrees with reasoning.

Nevertheless, it is far from being the case that all ships, with an equal degree of crowding, run the same risk in case of an outbreak of cholera. In this respect it is necessary to make a distinction between ships coming from a focus of cholera, that is to say, which have taken on board individuals who have resided a longer or shorter period in a locality where cholera prevails, and ships having on board a crew and passengers exempt from all cholera influence, and who are brought into relation with a locality or with individuals affected with cholera.

Among the first (those embarking at an infected port), in spite of the most unfavorable crowding, if cholera breaks out on board it ordinarily makes but a small number of victims, and this during the first days of the voyage, and, if this is prolonged, it dies out, not to appear again. Most frequently indeed, the cholera, properly so called, does not show itself.

The demonstration of this has been given in the most convincing manner by the last epidemic.

Among thirty-three steamers and one hundred and twelve sailing vessels arriving under suspicion of cholera during the last year at the Dardanelles, in the space of a month and a half, and coming for the most part from Alexandria, there were on board during the voyage but five cases of death, and about sixteen men affected with cholera, who were transported to the lazaretto. These ships carried together 3,058 men as crew, and more passengers, of whom 2,268 entered the lazaretto. The number of those who passed their quarantine on board is not given. This makes in all a total of more than 5,326 men, without counting the passengers who remained on board, who furnished five deaths and sixteen cases in addition, coming for the most part from steamers.* We shall see hereafter what happened at the lazaretto.

* The ships named as having had the cholera on board were the "Archiduc Maximilian," arrived on the 30th of June, two cases, of which one died; the "Mirra," 2d of July,

What we have said of the arrivals at the Dardanelles has been observed, in almost the same proportions, in all the ports of the Ottoman Empire. The report of Dr. Bartoletti on the course of the cholera in 1865 leaves no doubt on this point. We will add that the same has been the case wherever there were arrivals from Alexandria. It was especially so at Marseilles, where hardly any cases of cholera were reported on board the ships that brought, in a very short space of time, so great a number of fugitives into the city.

The same thing was observed at the beginning of the Crimean war. The ships which brought from Marseilles the first infected troops which distributed the cholera counted but a very small number of cases during the voyage, notwithstanding the enormous over-crowding.

In 1832, at the moment when cholera was raging in England, among the numerous vessels which transported thirty-three thousand passengers to Quebec, there were but two, "The Garrick" and "The Royalist," which had cases of cholera on board during the passage.

It is then a general fact that ships coming from an infected locality, and having on board persons who have sojourned in that locality, are not often the scene of any manifestation of cholera, and that if the disease breaks out in them, it ordinarily does not extend far, even where there is overcrowding. It is, on the other hand, an established fact that it is the ships which sail during the rise of an epidemic which show the largest number of sick.

There was during the last year an exception to this rule, if credit is to be given to the accounts from Egypt. The captains of the vessels which conveyed the pilgrims from Djeddah to Suez declared, on arriving, that they had had no cholera on board during the passage. Now, this statement has been acknowledged to be false. But they go farther, and declare that one of these vessels, the "Sidney," which carried 2,000 pilgrims, had lost more than 100 of them dur-

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one death; "Charkié," 7th of July, one case; "Minia," 8th of July, two deaths; "Djaférieh," five cases disembarked on the 14th and 15th; the "Tamise," 22d of July, two cases; the "Eiling," a sailing vessel, 22d of July, one death. The other vessels, making up the sixteen sick disembarked, are not mentioned.

ing the voyage.* This is simply a false statement, which has not been verified. May we not believe that there has been dissimulation on the one part, and exaggeration on the other? However this may be, this would be but a very remarkable exception; and the Commission does not pretend that there have not been exceptions to the rule laid down above. We might cite a certain number of them, susceptible of different interpretations, but which do not weaken in any respect the general rule. †

With regard to ships crowded with people entirely free from all previous cholera influence, if the disease manifests itself on board, it is the rule, that it takes on a rapid development; that the disease assumes a more fatal character than in the preceding instances, and runs through all its phases in a short time, and, up to a certain point, in proportion to the amount of crowding.

The history of the epidemic of cholera which raged on board the French fleet in the Black Sea, in 1851, offers a remarkable example in support of this proposition. This epidemic, for an excellent history of which we are indebted to Dr. Marroin, Surgeon-in-chief of the fleet (Paris, 1861), shows us the cholera making its entry into the Black Sea, on the 13th and 14th of July, with the "Primanguet" and the "Magellan," which had sailed from Gallipoli. The importation took place first at Varna, from whence the disease extended to the army on land. Up to the 22d of July, with the exception of the two ships in question, the fleet, principally at anchor at Baltchick, remained free from contamination; but, from that date, cholerine and occasional cases of cholera began to show

* The captain of the "Sidney," when interrogated recently at Djeddah, declared that last year he had thrown into the sea only eight bodies of cholera patients during his passage to Suez. — *Official document.*

† The most remarkable, perhaps, of these exceptions is that of two sailing vessels, the "North Wind" and "Persia," which, leaving Singapore in December, 1864, for Djeddah, lost by cholera, during the voyage, the one fifty men, out of six hundred and thirty-two, and the other ninety-three, out of five hundred and twenty persons on board. But the question in dispute is, to know precisely whether the cholera broke out on board these ships during the passage from Singapore to Mokalla, — in which case they imported the cholera into this last locality; or whether, on the contrary, they did not contract this disease during their stay at Mokalla. Under this last supposition, these ships would come into the category of those which have not yet been exposed to the choleraic influence, and would present nothing remarkable.

themselves on board many vessels. It continued so up to the 7th of August, the day when the division Bosquet, affected with cholera, went into camp at Baltchick. Frequent and necessary communications took place between it and the squadron. Two days after, the cholera broke out with extreme violence on board the fleet. It must be added (for, in such a case, we should tell everything, at the risk of furnishing weapons for an opinion which we do not admit), that two vessels, the "Friedland" and the "Jean-Bart," returning from a cruise on the coast of the Crimea, had had each a case of cholera on board, before holding any communication with the shore or with the squadron. This fact may be interpreted as you please: let us return to the vessels moored at Baltchick.

On the 9th of August, the epidemic assumed great proportions; in three days it had attained its greatest intensity, and at the end of ten days it ended. In that space of time, the five vessels the most severely affected had lost in all four hundred and fifty-six men by cholera; and, in eight days, the entire fleet, in an effective force of thirteen thousand marines, counted eight hundred deaths. From this moment up to the end of the war, there were on board the French fleet only isolated cases of cholera, and small, transient outbreaks, observed principally on board ships which transported troops not yet acclimated.

Any one who will take the trouble to consult all the known facts will see that all, or almost all, the very destructive epidemics of cholera on board ships have occurred in those which were carrying a great number of men, who had not, before embarkation, been exposed to the influence of a cholera atmosphere.

But, from the distinction which we seek to establish on experience, it must not be concluded, that those ships which have sailed from an infected place, and which have finished their voyage without cases, or with a few cases only, more or less characterized, — that those ships which thus present themselves with an apparent exemption, are really free from the danger of importation. No: this would be a dangerous error. One might have believed, heretofore, with an appearance of reason, that this was the case; but the facts observed during the past year do not admit of it.

The great majority of the ships departing from Alexandria did not have the cholera on board during the voyage. Did they any the less propagate the disease, even in the absence of all cholera cases on board? How, under these circumstances, did they propagate it? We shall not undertake to speak with precision, but it is certain that they did propagate it; for the *decisive* reason, that cholera *only* showed itself at the places where they arrived.

Here the question presents itself which has been raised by the development of the terrible epidemic of cholera which raged during the last year at Guadaloupe. One of two things happened: either the cholera was spontaneously developed in that island (and this would be a fact up to this time without example, outside of India), or the cholera was imported. But how? Was it by the winds across the Atlantic, when we know that cholera does not cross even a desert much less extensive? Was it not rather by means of a vessel? What vessel was it, then? Was it, as has been claimed from the first, the "Virginie," which sailed from Marseilles on the 3d of September (that is to say, during the epidemic), and arrived at Point à Pître on the 9th of October, after thirty-six days passage, without having presented, we are assured, any trace of cholera on board? Let us remark that cholera did not break out in Guadaloupe until the 22d or 25th of October, at the time when they were discharging this ship. Or was it rather, as has been affirmed since, by the ship "Sainté-Marie" that the importation took place? This last had left Bordeaux on the 15th of September, with a clean bill of health, and at that time there was no cholera in that city. But the version which attributes to her the importation admits, that some sailors who had come from Marseilles had embarked on board this ship, and that two men had died of cholera on board during the voyage from Bordeaux to Point à Pître, where she arrived on the 20th of October, and that the disease was communicated first to two washerwomen who had washed the soiled clothes of the crew of the said vessel. Now official information tells us, that the two sailors in question did not come from Marseilles, and that there had been no cholera on board this ship. This version then loses all its value.

There remains the first supposition, that the disease was im-

ported, after a long voyage, by a ship coming from an infected place, without having had a case of cholera on board. Whatever may be the accuracy of this version, of the value of which the Commission is not in a position to pronounce, it none the less follows, — *and this is the capital fact*, — that cholera did not break out at Gaudaloupe until after an arrival from a place infected with cholera. The circumstance, that similar arrivals elsewhere have not been followed by the same result, proves nothing against importation in this case; it proves only, either that the analogy was not complete, or that conditions favorable for transmission did not present themselves.

To sum up, *the Commission answers, that the intensity of epidemics of cholera on board ships crowded with men is, in general, proportionate to the crowding; and is so much the more violent, all other things being equal, if the men have not started from a focus of cholera, where they had been residing; that, on board crowded vessels, the course of epidemics of cholera is ordinarily rapid; finally, the Commission adds, that the danger of importation by vessels, and of giving rise to a grave epidemic, are not entirely subordinate to the intensity, nor even to the existence, of cholera symptoms on board during the voyage.*

Adopted unanimously, except by M. Monlau, who refrained from voting.

XXII.

What influence does the accumulation, in lazarettos, of individuals coming from a focus of cholera, have upon the development of cholera among the people detained at quarantine and in the neighborhood?

The question of the influence, upon persons at quarantine who have come from a place infected with cholera, of accumulation in a lazaretto, is one of the most interesting to be studied: it meets a fear, often expressed, of the danger of seeing cholera make great ravages among people thus detained. We are able to answer at once, that this fear is not at all justified by facts. It happens in lazarettos exactly as it does on board ship, — that is to say, when individuals, who find themselves thrown together, have previously

undergone the cholera influence, they possess a sort of immunity, over which the bad effects of crowding do not, in the immense majority of cases, exert any influence. The following facts prove this; they are from the work of Dr. Bartoletti on the course of the cholera in 1865:

At the time of the last cholera epidemic, the greater part of the Turkish lazarettos were crowded to excess with the mass of fugitives who resorted to them from places infected with the disease. We have only been able to obtain exact accounts from eleven of these lazarettos, but they are the most important ones; and the results which they give are so much the more important as all these lazarettos contained cases of cholera among the people detained at quarantine. In many, the crowding was carried to very great excess, principally at Salonica, at the Dardanelles, at Beyrout and at Trebizond. These eleven lazarettos received together 25,819 people held in quarantine, among whom there were four hundred cases of cholera, of whom two hundred and thirty-eight died. This would give, taking count only of the deaths, a proportion less than one in a hundred. It must be added, that many of these cases did not occur in the lazarettos, but that some individuals affected were sick when they disembarked. Thus, in nine lazarettos where the fact was noted, we find that, out of one hundred and eighty-five sick, of whom one hundred and one died, sixty-seven were affected with cholera when they disembarked, — which reduces very decidedly the number of cases which one might be inclined to attribute to the crowding in the lazarettos.

The following table, drawn up from the facts collected by Dr. Bartoletti, supports our position:

TABLE.

Indicating the number of quarantined persons admitted into the principal Ottoman lazarettos during the epidemic of 1865, *with the number of cases of cholera and the number of deaths.*

Lazarettos.	Number of people at quarantine.	Cases developed before entering the lazarettos.	Cases developed in the lazarettos.	Whole number of cases.	Number of deaths in the lazarettos.
Dardanelles	2,268	16	6	22	15
Smyrna	1,701	0	14	14	9
Salonica.	4,257	?	?	265	122
Volo	2,265	5	57	62	23
Beyrout	3,200	?	?	30	15
Cyprus	1,199	19	3	22	7
Crete	778	3	11	14	10
Benghazi	812	0	1	1	1
Trebizond	5,073	1	20	21	19
Samsoun	3,170	18	6	24	12
Bourgas	1,096	5	0	5	5
Totals	25,819	67	118	480	238

It will be noticed in this table, which should not be regarded as giving a rigorously exact report, but only as a simple sketch, that the number of cases developed in the lazarettos, and the number of deaths, is not always in proportion to the number of quarantined people; that, at Benghazi, for example, there was but one case among eight hundred and twelve quarantined people, while at Volo there were fifty-seven among 2,265, — so that it is impossible to conclude anything with regard to the influence of crowding upon the number of cases. It would be necessary to have, to settle this point, details, — which are unfortunately wanting, — concerning the condition of the quarantined people in each lazaretto. These

details have not been given for the most part, in spite of an express circular on this subject from the Ottoman government.

The office of the Dardanelles only has furnished upon this point almost all the information desired. We see that there nearly nine hundred persons, held in quarantine, were shut up at the same time in the lazaretto, causing a considerable crowding in proportion to the capacity of the establishment. In this state of things, which did not admit of conveniently separating from each other the different classes of suspected persons, there were nevertheless, among 2,268 quarantined people, from the 29th of June to the first days of August, but six cases of cholera developed within the lazaretto. Nevertheless, as will be shown hereafter, the disease extended from this establishment to the town, and made cruel ravages.

It would have been very interesting to know how the case was at Salonica, where, in a very short space of time, from the middle of July to the middle of August, there arrived a crowd of fugitives from Constantinople. The lazaretto, near to the town, was so crowded that the alarmed population required the removal of many persons who arrived,—who went, some of them, to Volo, — and obliged also the local authorities to establish a new lazaretto at a great distance from the town. But how many cholera patients disembarked at the first lazaretto, — for it was there that almost all the cases occurred? How many cases originated in the establishment? This is what the documents do not tell. We know only that among the dead were nine persons attached to the service of the lazaretto, — an enormous number when considered in relation to the number of persons in quarantine.

However the case may be, it none the less results from the preceding table; and this is all that we pretend to draw from it, that cholera has had but a very feeble development among people at quarantine, in the Ottoman lazarettos, although in most of them the accumulation of such people has been considerable.

It remains to be seen now what happens in localities more or less near these lazarettos.

In the Dardanelles, at Smyrna, Cyprus, Beyrout, Trebizond, Kustendje, Sulina, the cholera spread from the lazaretto to the

neighboring town, or at least it was developed consecutively to the admission of cholera patients into the quarantine establishment. It would be a matter of the greatest interest to be able here to say precisely in what manner things have happened at each of the above localities; but the want of precise information does not permit this. It is easy to understand, also, how many interests are opposed, in most of these cases, to the exact truth being known. Still, at Smyrna, at Larnica (Cyprus), and, above all, at the Dardanelles, it was possible to follow the chain of facts, and to demonstrate how the disease passed from the lazaretto into the town.

See how things happened at the Dardanelles. From the beginning of July, many cases of cholera had been admitted to or developed in the lazaretto, when, on the 12th, a soldier of the guard, at the gate of the establishment, was attacked by the disease. He was transported to the neighboring hospital, where he rapidly sank. On the next day, eight cases of cholera occurred; namely, two among the soldiers of the guard at the gate of the lazaretto, three in the garrison of the fort adjoining the establishment, one in the town, distant an hour's walk by land, but much nearer by sea, in the person of a *garde de santé*, who had left the lazaretto two days before, one in another quarter of the town in an individual who went every day to the lazaretto to sell cakes, and finally one in the person of a boatman of the Health Office. This was the point of departure of the epidemic which developed itself in the town, and did much mischief there, as may be seen by the following figures. In a population of eight thousand souls, reduced rapidly to six thousand by emigration, there were from the 12th of July to the 2d of September, three hundred and forty-four deaths from cholera, not including twenty-five deaths among the garrison of the forts, or nearly six per cent of the population.

In this case, therefore, the mode of propagation is not doubtful; and it is to be remarked, that while the 2,268 persons at quarantine counted but six cases developed in the lazaretto, the town was, on the contrary, cruelly tried. Must we not see the proof that the people at quarantine enjoyed relatively a sort of acquired immunity?

At Smyrna, the propagation from the lazaretto to the town is less evident. The 23d of June dates the arrival of the first vessel from Alexandria without a clean bill of health, and having disembarked a case of cholera at the lazaretto. But it must be said that before this arrival there had been other arrivals from Alexandria since the appearance of cholera in that city, which had been admitted at once to free *pratique*, conformably to the regulation, having had nothing suspected on board, and having been five days on the passage. Although this was the case, the first case of cholera observed in the city of Smyrna occurred on the 24th of June, in the person of an Armenian woman; other cases followed, few at first, until the 11th of July, but sufficiently numerous afterward to produce, at the height of the epidemic, from the 30th of July to the 7th of August, a mortality of eighty persons per day.

We cannot then affirm, that at Smyrna the cholera was propagated from the lazaretto to the town, although this establishment was contiguous; but it is none the less remarkable, that the people at quarantine suffered much less from cholera in proportion than the inhabitants. Thus it is estimated that the mortality in the city was about 2,500 in a population reduced to 100,000, or one in forty; while among the 1701 at quarantine it was but nine, or one in one hundred and eighty-nine.

All that we know of Larnica is, that the lazaretto there is very near the town, and that, from June 24 to July 18, 1,199 quarantined people were admitted there, among whom, nineteen before and three after their admission were affected with cholera. On the 7th of July, the first case occurred in the town, that of a coffee-house keeper who had lodged many persons on their discharge from quarantine. From that spot the disease spread to the whole town and subsequently throughout the whole island.

At Beyrout, the information which we have received shows, that from June 17 to July 25, 3,200 persons coming from Alexandria were subjected to a quarantine of five to ten days, either in the lazaretto, — a well-ventilated establishment, but too small for so many people, — or in isolated houses on the shore, on the other side of the town. The crowding was nevertheless considerable, and the time came when the people in quarantine could not be

restrained, and openly passed beyond the limits. But cholera existed already in the town. The first case had occurred on the 1st of July, in a man named Farrah, who had had no apparent communication with the lazaretto where the cholera cases were. The second case in the town occurred on the 3d of July, in the person of the Abbé Viale, the secretary of the Patriarch of Jerusalem, on the very day of his exit from the lazaretto where he had been passing ten days of quarantine. He died in a few hours. Notwithstanding this fact, it is not possible to establish with certainty any connection between the lazaretto and the first attacks in the town. The epidemic which followed was not very severe at Beyrout, as in the space of three months, in a population reduced to 50,000 souls by emigration, it caused but five hundred and ninety-three deaths, or one to eighty-eight of the inhabitants; but still here also the proportion is much greater than among the people at quarantine.

With regard to Trebizond, the documents do not permit us to come to any precise conclusion; although cholera had but a very feeble development in this town, which, the year before, had suffered so much from typhus.

At Salina, the cholera was very destructive, and there had been two cases among the people at quarantine confined in the lazaretto, when the disease broke out in the town. This is all we can learn from the information supplied us.

To sum up, at the Dardanelles the spread of the cholera from the lazaretto to the town is incontestable; it is very probable that the same happened at Larnica, and it is doubtful with regard to the other places mentioned above.

Of the other localities comprised in this table, six were spared by the cholera, and two of them, Salonica and Volo, deserve a special mention.

At Salonica, whither, as we have seen, the fugitives had flocked, circumstances were very favorable for the propagation of the disease, — namely, a considerable crowding (as many as 1,300 people at a time at quarantine) in the lazaretto, situated near the town, and a great number of cholera patients (two hundred and sixty-five) in the establishment. It is true, that we are told the cholera patients were carefully isolated from the other inmates of quarantine

(one can easily imagine what such an isolation must have been), and that, later, the lazaretto was transferred to a great distance from the town; but it is none the less certain that all the cases of cholera occurred in the first lazaretto, and that, in spite of the separation, the cases of cholera were more numerous at Salonica than anywhere else. There is also something more to be said. An individual who had passed out of quarantine, after having been detained there fourteen days, was, according to an account worthy of credit, taken four days later with cholera in the town. This attack, which was fatal, was followed by two other cases, of which one was mortal, in the same house, which was immediately placed in quarantine. Although this happened, Salonica was spared. Whatever explanation may be given, the fact is worthy of record. In the mean time, many villages in the neighborhood of Salonica, whither people had gone after leaving the lazaretto, had not the good fortune of that town; the cholera appeared among them with considerable intensity.

Volo received, to some extent, the overflow of quarantined persons destined for Salonica: 1,649 of the 2,265 passengers on shipboard were there disembarked on a desert island, and placed under tents. Sixty-two cases, of which twenty-three were fatal, occurred among them from July 26 to August 10. The town was spared, although the Clerk of the Health Office was attacked and died outside of the lazaretto, and the physician of that establishment, attacked himself, took refuge in the town; but the disease appeared in many villages distant from five to ten miles from Volo, where, forty-two days after the last case observed in the lazaretto, an attack occurred in an individual coming from one of these villages. Thus, in spite of certain contraventions, the epidemic spared the town of Volo.

We will finish this history with some details concerning Rhodes and Crete.

Rhodes received no less than 2,618 quarantined persons in the space of two months, beginning with June 19. On account of the limited capacity of the lazaretto, they were installed, for the most part, in good condition, in tents. On the 20th of June, an inmate of quarantine, having landed in the evening from an Egyptian

barque from Alexandria, which had no cholera on board, fell sick of cholera, and died the same day. The other passengers, to the number of eighty-six, were transferred elsewhere and placed under tents, and passed ten days of quarantine without any case. This was the only case observed at Rhodes among the people at quarantine. The island was entirely spared.

The island of Crete, in the space of about two months, received nine hundred and seventy-two suspected persons (without counting the crews), among whom one hundred and eighty-four remained on board and seven hundred and eighty-eight passed their quarantine on three islands of the Gulf of Sude, where tents and barracks had been put up. The surveillance was very severe; all communication between one island and another was interdicted. The quarantine was for ten days, and, in case of cholera, it was prolonged for ten days after the case occurred. Two vessels arriving from Alexandria brought in them some cholera patients. One, arriving on the 28th of June, had had two fatal cases in port. The passengers having landed, to the number of two hundred and fifty, had among them during their quarantine, eight cases and four deaths, to which must be added that of one of the Health Guard. The other vessel, a Turkish brig, arrived on the 27th of July with fifty-eight passengers, mostly workmen from Suez. There had been five cases during the voyage. One only of these was disembarked at the lazaretto. Among this group, from the 7th of August to the 3d of September, there were five deaths from cholera. What was most remarkable to note was, that not only the cholera did not penetrate into the island of Crete, but there was no case among the people at quarantine, except those of the two groups of which we have been speaking, and which, it is true, had been set apart on two different islets.

We shall carry these citations no farther. It is proper only to add, that the length of quarantine in the Turkish lazarettos was not always the same; that, at first, it was from five to ten days, conformably to the regulations, according as the vessel had or had not had cases of cholera during the voyage; and that soon it was extended to ten full days in all cases, and even longer in certain

localities. This is not the time to dwell upon the value to be attached to this duration.

From all which precedes, the Commission concludes *that the crowding together of people coming from a place where cholera reigns, in a lazaretto, has not the effect of producing among the inmates of quarantine a great extension of the disease; but that such a crowding is none the less very dangerous for the neighborhood, as it is calculated to favor the propagation of cholera.*

Adopted unanimously except by M. Monlau.

XXIII.

What influence do great collections of men in armies, fairs, pilgrimages, exercise upon the development and propagation of epidemics of cholera?

The remark made with regard to vessels is applicable to all great collections of men, with some variations arising from the different conditions under which they are placed.

When cholera penetrates a body of troops, in a compact army, up to that time free from all cholera influence, it develops rapidly and produces ravages according to the hygienic and moral conditions of that army; and the epidemic runs through, although less rapidly than on board ship, all its phases in quite a short time. It also dies out promptly unless new arrivals, troops as yet unaffected, come to join them, and to keep up the disease by producing fresh outbreaks. In this case the former suffer much less in proportion than the new comers, in consequence of a relative, acquired immunity. Numerous examples might be produced in support of these propositions. We shall content ourselves with citing that which was observed in the French army during the Crimean War.

The first invasion of this army by cholera, at Gallipoli and Varna, was terrible; but it was in some sense a hurricane, which, after a month, left only traces here and there. The disease, however, did not entirely disappear until towards the end of the war, and it showed from time to time fresh outbreaks, which corresponded always with the arrival of fresh troops. These always paid a tribute more or less large; a small number of the older

troops, among the *malingerers*, were attacked, and the disease slumbered.

One very remarkable example will show how fresh troops may reanimate an epidemic which one might have believed extinct. At the beginning of April, 1855, there arrived from France at Constantinople from fifteen to twenty thousand men, belonging in part to the Imperial Guard. These troops had not had a single case of cholera among them during the voyage. They were encamped on the heights of Maslak, a place perfectly well chosen in a hygienic point of view. At this time there were in the city of Constantinople but very few cases of cholera. The reports of the French military hospitals give but fifty-three cases of cholera for the month of March. The report of the 10th of April did not give a single one. In the Crimea the cases were equally rare at this time. The troops in question were hardly installed at Maslak, when, on the night of the 14th and 15th of April, cholera broke out among them. It should be said that, for some days past, cases of diarrhœa had become numerous in the camp. A severe epidemic followed, which declined rapidly, but which accompanied these troops into the Crimea, where they arrived in the beginning of June, and where their arrival was marked by a serious outbreak of cholera. It must be added also, that from the camp of Maslak the disease extended to Pera and to those villages on the Bosphorus the nearest to the cholera focus. (*Extract from official documents.*)

As for the propagation of cholera by armies or by bodies of troops in movement, it is a fact too well known to require much argument here. It is sufficient to recall the war in Poland in 1831, which was the great cause of the rapid propagation of cholera in Europe; the circumstances of the civil war in Portugal in 1833 (*Gomez*), when cholera was transmitted to the province of Algarva, and to the towns of Torres-Vedras, Caldas, Leiria, and Coimbra, by the movements of troops. According to information communicated by Dr. Lenz, during the epidemic of 1847 and 1848 in Russia, cholera was imported many times into places previously intact, by bodies of troops coming from a contaminated locality. Thus, in 1847, the disease was transmitted to Kisliar by a troop of soldiers coming from Temir-khan-Choury; in 1848, in

the district of Smolensk, by two regiments of hussars coming from Moscow, and communicating the cholera, not only to the villages where they had left their sick, but even to villages where they had made but a single halt for the night. The same thing has been observed in the movement of troops in India. In the governments of Nijni-Novgorod, of Kostroma, of Jaroslaw, and of Vladimir, all the local authorities attributed the rapid propagation of cholera in the spring of 1848, at the time of the opening of navigation on the Volga, to the masses of men employed in towing the boats, and who, when the first were attacked, fled in all directions.

With regard to fairs, they have, like all large gatherings, the effect, when cholera appears, of creating great foci of infection, with this aggravation, — when compared with armies, which, at least, always remain more or less compact, — that when this crowd tainted with cholera disperses in all directions, it tends to propagate the disease everywhere. We may cite, as having produced this result, the fair at Tuy, which, in 1854, propagated the cholera in the province of Minho in Portugal (*Gomez*); that of Samara, from which the cholera was propagated in the government of Orenbourg (*Lenz*); the great fairs of India; and, notably, that of Hurdwar, which has been considered in connection with pilgrimages; finally, that of Tantah, in Egypt, which, in 1848, contributed much to the spread of cholera. Nevertheless, with regard to this fair of Tantah, we ought to say that this year, having taken place a short time after the epidemic of cholera, it has had no unfavorable effect upon the public health, thus confirming what we have advanced above concerning the relative immunity which a crowd may enjoy which has just come from undergoing the trial of a cholera epidemic. (*Communication of Dr. Salem Bey.*)

Finally, with regard to pilgrimages, we have spoken of them sufficiently in detail, in connection with the cholera in India, to show the important part which this kind of gatherings plays in epidemics of this nature. We propose also to return to the subject, a little farther on, in connection with that of Mecca.

Great gatherings of men, therefore, contribute much to the rapid development of epidemics of cholera; they constitute foci of reinforcement, and, by their dissemination, their migration into locali-

ties hitherto healthy, they favor the propagation of the disease; for if a single case of cholera imported into a healthy locality is sufficient to develop an epidemic there, as was proved in the case of Altenburg, so much the more probable will such a development be from the arrival of hundreds of contaminated people.

The commission concludes then that great gatherings of men (armies, fairs, pilgrimages), are one of the most certain means of the propagation of cholera; that they constitute great epidemic foci, which, whether they march after the manner of an army, or whether they are scattered, as in the case of fairs and pilgrimages, import the disease into the countries which they traverse; that these gatherings, after having been subjected ordinarily in a rapid manner, to the influence of cholera, become much less susceptible to its power, and that it disappears very speedily, unless new arrivals come to keep up the disease.

Adopted unanimously.

XXIV.

What is the influence of dissemination on the intensity and development of epidemics of cholera?

What has been said heretofore shows, that the dissemination of cholera foci in healthy localities is an almost certain means of propagating the disease, and we need not, therefore, return to the subject; but, on the other hand, experience shows that dissemination, applied to an assemblage which cholera has penetrated, is calculated to diminish the violence of the epidemic, to diminish the number of cases in this mass, provided that the dissemination is not too slow, and that it takes place as soon as the disease appears. Dissemination, in this case, diminishes the chances of propagation in the whole mass attacked; but it should be understood, that most frequently, in consequence of the conditions under which it takes place, it only retards the course of the epidemic; so that when we come to compare results, we find that in this disseminated mass, in which individuals are more or less separated one from the other, but always in communication, the mortality is almost the same as in a compact mass; only it requires a longer period of time. It is evident that in many cases the rapid extinction of an epidemic

which has committed great ravages in a crowded mass, has been wrongly attributed to the effect of the dissemination of this mass; whereas it was but the consequence of the natural evolution of cholera epidemics in such cases.

Still, dissemination, made at the right time, has given favorable results which cannot be denied, and had it no other effect than to ameliorate the hygienic conditions of individuals, cannot be too highly recommended.

But it should be well understood that this dissemination ought not to take place into healthy localities, where it would cause the introduction of the disease, at the same time that it would be no advantage to the contaminated individuals; it ought, on the contrary, to take place within the limits of the locality where the infection is manifested.

Consequently the Commission concludes, that the dissemination of a compact crowd, at an opportune time, may render less violent an epidemic of cholera which has appeared in the midst of it, and even arrest its extension; but that this dissemination, on the other hand, gives rise to great danger of propagating it, if it take place in a region as yet unaffected.

Adopted unanimously.

XXV.

What is the part which belongs to the pilgrimage to Mecca, in the cholera epidemics of our day?

The influence of the pilgrimage to Mecca, so far as the crowding is concerned, is already sufficiently indicated. Like all other pilgrimages, this one consists of an assemblage of individuals from very diverse countries, and consequently, for the most part, of people free from all cholera influence until the moment of their arrival at the Hedjaz. This last circumstance is the reason why, if cholera manifests itself in this mass, ordinarily a violent epidemic rapidly follows,* and so much the more destructive as this agglomeration of

* This proposition would seem to be contradicted by the report of the English Consul at Djeddah, who, during the six years of his stay in that town, has always seen cases of cholera there at the moment of the return of the pilgrims; but were these cases of true Asiatic cholera, or sporadic cases, such as are ordinarily observed in these countries during the warm season?

men finds itself in the midst of hygienic and climatic conditions of the most unfavorable character. The renewal each year of individuals collected at Mecca, on the one part, and on the other the habitual arrival of pilgrims coming from contaminated places, accounts for the frequency of epidemics since 1831. The dispersion of the pilgrims, when once the epidemic is developed, constitutes a danger of propagation, so much the greater as it takes place rapidly and in the warm season. It was thus that during the last year, as we have seen, the rapid dispersion of the pilgrims transported by steamboats, led to the rapid propagation of cholera in Egypt at first, and afterwards its distribution in the basin of the Mediterranean. But, on the other hand, as has been said above, the dissemination of the pilgrims, and their journey by caravan across the desert, through countries almost uninhabited, far from favoring the propagation of the disease, is, on the contrary, one of the best means of extinguishing it.

Now, with regard to the influence of the pilgrimage to Mecca on the epidemics of cholera, which have followed, particularly in Egypt, it is to be remarked that, of five epidemics which have desolated this country since 1831, two only have coincided with the return of the pilgrims, and then, when this return coincided with the warm season: in 1831, in July, and in 1865, in June. Of these two epidemics, the first, which commenced with their return by sea, was probably imported by them, although the fact has not been clearly demonstrated; but the second very certainly was.

Thus, then, twice only, with an interval of thirty-four years, cholera has been imported into Egypt by pilgrims returning from Mecca by sea; although, during this period, the disease has raged many times at Mecca, at the time of the pilgrimage; but, in this connection, it should be remarked that the transportation of pilgrims from Djeddah to Suez by steamboats did not take place until 1858. This infrequency, then, is no guaranty for the future.

In conclusion, *the part of the pilgrimage to Mecca, as a propagating agent of cholera, with reference to countries bordering on Europe,* (the only ones with regard to which we have positive information) *has been the importation of this disease into Egypt twice, with an interval of thirty-four years, during the warm season.*

Adopted unanimously, except by M. Polak, who refrained from voting.

On the Influence of Hygienic Conditions.

XXVI.

What is the influence upon the violence of cholera epidemics of hygienic and other conditions of locality; in other words, what are the assisting causes of cholera?

We do not think it necessary to the object of our labors to dwell at length, here, upon all the conditions which favor most epidemic diseases; we shall only insist upon those which appear to have a particular influence upon the development of cholera.

Misery, with all its debilitating consequences, in respect of food, lodging, filthiness, crowding, etc., makes most individuals more susceptible to the influence of the greater number of epidemic diseases, but of none more than cholera. The predilection with which this disease falls upon communities, or that part of communities where misery reigns, is a fact too well known to need argument here. Next to misery come fatigue, excesses, which act in the same way, by weakening the organization, and all morbid conditions which diminish vital resistance, particularly those which act upon the digestive organs.

It remains to be said, in conclusion, that cholera attacks, by preference, individuals debilitated by any cause whatever.

The Commission has not thought it necessary to dwell upon the influence of alimentary regimen which, in time of cholera, acts in two ways; either, through insufficient nourishment, by debilitating the organism and rendering it more apt to contract the disease, or by the injurious action upon the digestive canals of substances recognized as indigestible, and especially green fruits, etc., producing troubles which favor the explosion of cholera.

We need not repeat what we have already said upon the influence of crowding; we will only add, that over-crowding, combined with misery and all which follows in its train, constitutes a condition the most calculated to augment the violence of a cholera epidemic.

We pass to influences of a different kind.

Temperature and climate. Although cholera has not been excluded by any climate nor by any temperature, it is nevertheless established by observation that in general the more or less rapid evolution of a cholera epidemic and the extent of its course, are in relation with the elevation of the temperature; that thus the warm season accelerates at the same time the evolution and the march of the epidemic, while winter retards them, and sometimes arrests them. This is a general rule, noticed in all countries, which the researches of Hirsch have confirmed. Yet we have seen, as an exception, a rigorous winter fail to arrest the ravages of a cholera epidemic, at Moscow and Oremburg, for example; but should we not attribute these exceptions, in cold climates, to the manner of living during winter, to the mode of heating, to the want of ventilation in the dwelling-houses? The only example showing that a very rigorous winter does not always oppose an obstacle to the invading march of an epidemic of cholera, was observed in Russia in 1830–31, during the troubles in Poland, and would seem to be due to the movements of great masses of troops towards the theatre of war (*Lenz.*)

Air. No one doubts that confined air in general, or air vitiated by exhalations from animal or vegetable substances in a state of putrefaction, predisposes the organism to contract cholera, and also helps to make the disease more destructive; but in the case of an epidemic, confined air acts also and especially, as will be shown hereafter, as a vehicle of the cholera principle.

Water appears, according to observations made principally in England by Dr. Snow, and in Germany, by Dr. Pettenkofer, to contribute under certain circumstances, to the development of cholera in some localities. This occurs when water is charged with organic detritus, like that of rivers which traverse great cities, or that of wells which receive the infiltrations of a porous soil impregnated with matters in a state of decomposition, or which communicate with drains or privies. In the time of a cholera epidemic, these waters, according to Snow, are charged also with the morbific principle, and, used for domestic purposes, they may

propagate the disease. This opinion is based upon an observation made in London, where, in 1848–49, the mortality was 12.5 to 1000 of the inhabitants, who made use of water, from the Thames, taken by the Lambeth Company in the interior of the city, and where, in 1854, there were but 3.7 deaths to the thousand persons using the water taken, by the same company, above the city; while the quarters which continued to use water taken *intra muros* had thirteen deaths to the thousand inhabitants (*Simon*). Dr. Snow has cited also, as a characteristic fact, the great mortality from cholera which affected exclusively, in Broad Street, those persons who used the water from a certain well which received the infiltrations from a sewer. This water, transported to a distance, we are assured, communicated cholera to a person who drank it. Similar facts have been noted elsewhere.

Water in this case, as air in the preceding, must have served as the vehicle for the introduction of the morbific principle into the system.

But it is elsewhere, according to most German authors, and especially according to Dr. Pettenkofer, who has made this question the subject of the most interesting researches, — it is in the privies, the sewers, in the soil itself that we should see the principal receptacles of the cholera principle. Although the conference has not given itself up to theories, the doctrine of Pettenkofer is so well accredited, it is related to facts so important for prophylaxia, that we are not permitted to pass it by. It rests upon this proposition, generally admitted, that the alvine dejections of cholera patients contain, in some form or other, the propagating principle of the disease. This proposition, already formulated in 1849 by Dr. Pellarin (*Gazette Médicale de Paris*), who insisted even then upon the necessity of disinfecting the dejections of cholera patients by sulphate of iron, was corroborated by the observations of Budd, published in 1854, and by those of Snow; and next, we may say, was settled beyond a doubt, by the researches of Dr. Pettenkofer. It is but the legitimate deduction from numerous facts which have established that latrines, privies, sewers, filth, are the principal receptacles from which the cholera principle is exhaled. The facts

are too well known to need enumeration here. These cloacæ, in relation to the whole population, play precisely the same part that we have been led to attribute to the linen and soiled articles among cholera patients; that is, being the natural receptacles in a certain sense of the alvine dejections of these patients, they become the propagating agents of the disease.

Dr. Pettenkofer goes much farther. He considers as demonstrated by his observations, that during an epidemic of cholera the soil itself of a locality where the disease prevails has a great deal to do with its development by the emanations which escape from it. He claims as proved that a porous soil, easily permeable to water and air, and charged with excrementitious matters (by consequence especially an alluvial or a marshy soil), when impregnated with cholera dejections, becomes first a receptacle, then, according to circumstances, a focus, more or less active, from which the principle of the disease is disengaged. The activity of this disengagement will depend upon the level of the subterranean waters, and, consequently, will be in relation with certain alternations of the degree of humidity of the superficial layer of soil. This receptivity of the soil for the principle of cholera explains very well the tenacity of the disease in certain localities, and even its re-appearance, apparently spontaneous, after a more or less complete disappearance. It is in India, and particularly in the regions where cholera is endemic, that it would be most convenient to verify the exactness of the doctrine in question. Perhaps in this would be found the explanation of endemicity. It is also a fact well known in India, and of which we find examples in authors, that the soil on which a troop of men (soldiers, or others), affected with cholera, encamps, is capable of transmitting the disease.—*Bengal Reports by Jameson*, 1819. *S. Rogers, Report on Asiatic Cholera, p.* 18, *London*, 1848. *Scott, Report on Cholera in Madras, p.* 112, *London*, 1849.

However it may be, whether this doctrine finds or not its complete justification in all cases, it appears well established that a porous soil charged with detritus, such as Dr. Pettenkofer describes, an alluvial soil, in one word, favors the development of cholera epidemics; and if all epidemics are not explicable by this

circumstance,* this does not weaken the rule, but only proves that other conditions are equally favorable for the development of cholera.

If all the hygienic conditions which have been detailed above may be considered, in general, as favoring cholera epidemics, there are nevertheless certain facts which show that conditions quite as unfavorable in appearance have not had a similar result. Many cases are cited where, in a city, certain infected quarters, which it would seem ought to be a prey to the epidemic, have been particularly spared. We will relate one instance of this kind which occurred the last year in Constantinople. At the time when cholera was raging at the arsenal, there were in a prison, situated within the limits of this establishment, seven hundred galley slaves, of whom five hundred went out every day to work. Now, the prison is a true cloaca, where are accumulated hygienic conditions of the worst kind. During the height of the epidemic, all the galley slaves were spared; and it was only at the time when it was declining at the arsenal, that one of the keepers of the prison was attacked. Soon after, fifteen cases occurred among the seven hundred galley slaves, and seven only were fatal. On the other hand, the soldiers and the marines of the arsenal lost one man in nine from cholera. Is it not very remarkable that when these last, placed in hygienic conditions relatively good, gave a mortality of eleven per cent of their whole number, the galley slaves gave but one per cent?

The Commission limits itself to mentioning these facts, which show that all has not been said with regard to the auxiliary causes of cholera.

In conclusion, *the Commission recognizes that the hygienic and other conditions which in general predispose a population to contract cholera, and, consequently, favor the intensity of epidemics, are: misery, with all its consequences, the accumulation of individuals,*

* Dr. Pettenkofer explains in a very ingenious way how entirely different conditions of soil may aid in forming partial depots of similar detritus. He explains also, by a very subtle theory in which we do not propose to follow him, how penetration into the soil in question of cholera matters, is not indispensable in order that exhalations from this soil may give origin to combinations which favor the development of cholera.

impaired health, the warm season, want of ventilation, and exhalations from a porous soil impregnated with organic matters, particularly if these matters come from cholera dejections.

In addition, the Commission is of opinion, that, as it appears demonstrated by experience that the discharges of cholera patients contain the generating principle of cholera, it should be admitted that sewers, privies and the contaminated water of a city may become the agents for the propagation of the disease.

The Commission adds, that it seems to result from certain facts that the soil of a locality, once impregnated with cholera detritus, is able to retain for a considerable length of time the property of disengaging the principle of the disease and of thus keeping up an epidemic, or even of regenerating it after it has become extinct.

Adopted unanimously, except by M. Pélikan.

Immunity with Regard to Cholera.

XXVII.

How is immunity from cholera to be interpreted ?

The Commission would not consider that it had faithfully performed its task, if, after having proved the transmissibility of cholera, and indicated, as far as possible, the conditions which favor its propagation, it took no notice of the resistance which certain countries, certain localities, and the majority of people oppose to its development. It is to this resistance, which is also very variable, that we give the name of immunity. It supposes that the locality which enjoys it, has resisted the importation of cholera, and that the individual who remains unattacked, has been exposed to the disease, as a physician is, for example, in the midst of an epidemic focus.

It is so much the more important to notice this immunity, since it has been invoked, although wrongly, against the transmissibility of cholera, and because, on the other hand, it leads to important considerations of prophylaxia.

Physicians who have believed that they here saw the proof that cholera is not transmissible, have forgotten that the same immunity, the same resistance is met with, more or less, in the case of all

diseases reported the most transmissible, or contagious, without exception. It is so with the plague, with yellow fever, small pox, scarlatina, etc. If it were otherwise, if all these diseases were transmitted by the simple fact that they are transmissible, and that there has been exposure to contagion, the human race would have long since disappeared from the surface of the globe. Happily, it is not so, and a well-balanced organism, opposes to all these diseases, an efficacious resistance in the great majority of cases. The principle of every transmissible disease, is not regenerated, except under certain conditions, without which it is sterile; just as it is not sufficient to cast a seed at random upon the ground, to reproduce the plant from which it came; it must meet there with all the conditions favorable for its germination. Undoubtedly, all morbific principles have not the same degree of virulence, but the necessity of an organism favorable to the regenerating evolution of a morbific principle, is none the less a fundamental point of the doctrine of the transmission of diseases.

Let us proceed to facts relating to cholera.

It is a matter of observation, that certain countries, certain localities have resisted completely or partially the importation of cholera, that this disease has become extinct there without propagating itself further. Among the countries, which, in Europe, have enjoyed this immunity, we may cite Alpine Switzerland, properly so called (except a certain number of cases in Argovia, Thurgovia, and Tessin in 1855,) which besieged to some extent by the disease, has resisted its invasion. (*Marc d' Espine, Archives Génerales de Médecine*, 1857.) The same has been the case with a certain number of mountain masses. We know the interesting researches of M. Fourcault in 1849, those of Messrs. Boubée and Vial in 1853, on the influence of certain geologic conditions upon cholera, and especially upon the repulsive influence of a granitic soil; from which the conclusion has been drawn, that a granite soil is an obstacle to the development of cholera. This conclusion expresses a fact generally true, but which is far from being without exceptions. We may say the same of altitudes: cholera has been observed at a considerable height, while it is a fact that in a given country high regions are more spared than low plains. Dr. Polak

has communicated to the commission the interesting fact, that when the cholera was raging at Teheran (3300 English feet above the level of the sea) the disease was propagated to villages situated not far from there, on the slopes of Elbourz, at the height of 6000 feet, where it only occurred in isolated cases; but it went no higher. During three epidemics in which these villages were attacked, the King of Persia having transported his camp, composed of more than ten thousand persons, to the valley of Laar at the height of 7500 feet, to the base of the volcanic peak of Demawend, the camp was entirely spared, notwithstanding constant communication was held with the infected localities.

But let us come to the more important facts of towns, which up to the present time have resisted more or less the cholera influence. These towns are numerous in Europe; But the one which in this respect deserves to be mentioned, above all, is the city of Lyons. This city of four hundred thousand souls, seems at first sight to combine all the conditions favorable for an epidemic of cholera. Situated at the confluence of two rivers, if, on the one side it is based upon an alpine soil, on the other it rests upon alluvium; it contains a large collection of workmen; the causes of insalubrity and misery are not wanting, furthermore it is on the grand line of communication which traverses France from south to north, and it has served as a place of refuge for large numbers of fugitives coming from places where cholera was raging. In spite of all this, Lyons has, so to speak, resisted until now the cholera influence. It entirely escaped the first epidemic, which, in 1832, ravaged the North of France, then that which ascended the Rhone in 1835. Later, during the epidemic of 1849, a barrack was invaded and some cases of cholera showed themselves in the neighboring quarters; but after three weeks it all disappeared. In the autumn of 1853, when cholera raged in the department of the Drôme, the disease appeared at Lyons, and reached the number of four hundred cases and one hundred and ninety-six deaths, after which it died out. Finally, in the year 1865, cholera has not been signalized, so far as we know, except perhaps by isolated cases, which have come from without. It cannot be said that Lyons has escaped because there have been no arrivals from cholera districts; far from

this, such arrivals have been very numerous; the disease has been imported there and has sometimes developed itself, just enough to show the power of resistance of the native population, or, if you please, the locality, to its propagation.

Does this fact, and many other analogous ones, prove that cholera is not transmissible? Not the least in the world; they prove only that there are localities, as well as individuals, which enjoy a certain immunity against its transmission; an immunity, which for these localities, may be complete, or partial, permanent, or temporary. We say temporary, for there are cases which show that a locality which has resisted at one time, has been invaded at another, and *vice versa*. These local immunities also, are not peculiar with regard to cholera; they exist equally for the plague and yellow fever.

As for the cause of complete, or nearly complete immunity, when, as at Lyons, this immunity appears under conditions which would be believed favorable for cholera, it escapes us.

It is only a fact, important to be established, and very worthy of serious study.

It is not the same with relative immunity more or less pronounced, which certain countries have always enjoyed, or only recently; this immunity, the general rule, when we look at it closely, may be explained by good hygienic conditions existing in these localities, or by notable improvements recently introduced. This relative immunity is a reply to those who are too much inclined to attribute the safety of populations against cholera exclusively to measures of quarantine; it proves, not that these are useless, — far from that, but that hygienic measures are the necessary complement of them.

With regard to the fact of the immunity of individuals plunged in the midst of a cholera focus, it is not less worthy of attention than that of localities.

We have seen from all which precedes, that, when cholera breaks out in a concentrated mass of human beings, it takes but a limited number of victims, variable according to the circumstances which we have enumerated; but which, under the most unfavorable conditions, has never gone beyond twenty per cent of the whole. We

may say that an epidemic of cholera in which the number of genuine cases amounts to five per cent of the population is a very grave one. This proportion was not reached even during the last year at Constantinople.

The resistance which the human organization opposes to the cholera poison, in the midst of a focus of infection, is, for some persons, complete, — that is to say, the organism in their case does not experience any appreciable trouble. In the greater number, the poison shows itself in a derangement of the health, which varies from a simple epigastric malaise, with borborygmi and tormina, to cholerine; but, in the immense majority of these cases, the organism resists, and an attack of cholera, properly so called, does not take place. Finally, in a number relatively limited, but very variable, the organism succumbs and cholera is declared. It is these last cases which furnish the measure of the gravity of an epidemic. This is what observation shows.

Complete immunity, then, with regard to the influence of cholera, is not the rule. Further, be it complete or incomplete, it may be only temporary; that is, an individual who is entirely refractory throughout the course of one epidemic may succumb to the next, and also an individual who has resisted during a certain period of an epidemic may become affected at last. It is this which is often enough observed among physicians, who, stimulated by the sentiment of duty, resist at the height of the epidemic, but succumb sometimes at its decline, when weakened by fatigue.

Immunity from cholera, then, is in definite proportion to the vital resistance of individuals, and is variable as that is. This is the reason why, with regard to immunity, the past is no guaranty for the future.

To this result of observation it has been objected, that, during epidemics of cholera, it is not unusual to see very vigorous individuals struck down by the side of persons in appearance very feeble, who have been spared; but physicians well know that vital resistance is by no means proportionate to muscular energy, and that such a nervous individual, of puny appearance, resists much better morbific influences than those colossi who are in reality but badly balanced phenomena.

Besides the immunity, more or less complete, proper to each individual, there is also a temporary immunity which exposure to a recent epidemic leaves behind. It is by this sort of acquired immunity only that we can explain — as we have seen above — the rapid extinction of cholera in the midst of crowded masses of men; and this is the reason why, at least for a certain interval of time of which it is not possible yet to fix the duration, a grave epidemic of cholera cannot be revived, in spite of new importations, in a locality which has just been tried by this malady. This, to be sure, is not applicable to places of pilgrimage, where masses of men, *constantly renewed*, periodically come together.

If, as we have seen, immunity from cholera is proportionate to the vital resistance, and if we refer in this connection to the auxiliary causes of the disease, we find that these causes are precisely of the nature of those which diminish the vitality of individuals, or of an entire population; and we arrive at this conclusion, that immunity from cholera results from all conditions opposed to these auxiliary causes, that is to say, good hygienic conditions; that the influence of these is such, that, if it were possible to make these conditions general, and in this way to reduce cholera to the proportions which it attains among classes and even whole populations in easy circumstances, it would be a disease of but trifling importance in its effects.

The Commission affirms then, that, in opposition to the transmissibility of cholera, there exists in a healthy man a resistance capable of neutralizing the toxic agent; and that this resistance, weakened among miserable populations and in individuals debilitated by any cause, may, by the increase of easy circumstances and by good hygienic measures, be generalized to the point of rendering cholera a disease but little to be feared. But, unfortunately, we are far from this, and it is for this reason that measures of isolation are, and will yet be for a long time, necessary.

It must be very well known that cholera, although transmissible, does not attack fatally all the individuals exposed to its influence; that a well-regulated life, good hygienic conditions, are almost certain guaranties against its action; that it rages by preference in unhealthy localities, among populations weakened by

misery and among individuals undermined by disease or excess.

The Commission, therefore, while proclaiming as an incontestable fact the transmissibility of cholera, believes that it is necessary to add the corrective which sets limits to its transmission.

It is more dangerous, in their opinion, to conceal a part of the truth on this point than to tell the whole of it.

In conclusion, *the immunity which certain localities enjoy, that is to say, the resistance, permanent or temporary, general or partial, opposed by these localities to the development of cholera within their limits, is a fact which does not exclude transmissibility, but which indicates that certain local conditions, not yet entirely determined, are an obstacle to the development of the disease.*

In the same way the immunity, more or less complete, and more or less durable, which the majority of persons in the midst of a focus of cholera enjoy, an immunity which attests the individual resistance to the toxic principle, is a circumstance to which we should attach the highest importance.

In the point of view of epidemic development, it is the corrective of transmissibility; and viewed with regard to prophylaxia, it sets in operation proper means to arrest the ravages of the disease.

Adopted unanimously, except by MM. Monlau and Pélikan, who refrained from voting.

Deductions with regard to the Properties of the Generative Principle of Cholera.

The Commission does not intend to occupy itself with the numerous hypotheses which have been put forth with regard to the nature of the cause which produces cholera; it only wishes to disengage, as corollaries from the facts which have been set forth in this work, certain attributes of the generative principle of the disease, the knowledge of which may lead to precautionary measures.

XXVIII.

From the facts which we have proved, and which relate to the genesis, the propagation and the transmission of cholera, can we draw

any precise conclusion with regard to the generative principle of the disease, or at least the media which serve as its vehicles or receptacles, the conditions of its penetration into the organism, the ways by which it passes out, the duration of its morbific activity, in a word, all its attributes, a knowledge of which is important in order to guard against it ?

Whether the principle of cholera be called contagion, a germ, miasm, whether we suppose it formed of an organic substance or not, it has always escaped all investigations, which have never been able to isolate it, and it is known to us only by its effects. In this respect, it does not differ from other morbific principles.

What we do know is, that it is reproduced in man by the fact of the morbid evolution to which it gives rise.

In our countries, we have never seen it originate in any other way: it is by successive regenerations in man that it is multiplied and propagated; never has the character of the soil, nor the worst hygienic conditions given rise to it. But at its place of origin, in India, where cholera is endemic, is the same true? Is the morbific principle there produced spontaneously outside of man, by means of conditions yet unknown, which exist nowhere else? Or, to apply the doctrine of Pettenkofer, is the soil of the places where it reigns as an endemic only the receptacle of the germ, and has it the property of retaining it long enough to permit that an incessant disengagement, more or less active, does not exhaust it before it is renewed? What we have said concerning the tenacity of cholera in certain localities of Europe gives some credit to this hypothesis. The Commission would merely indicate these important problems, which it has not the means of solving.

However it may be, the principle of cholera, of that which has invaded the whole world, seems native in the valley of the Ganges; and it is principally there, that, on an alluvial soil, low and humid, it maintains itself in our time in an endemic condition. From this fact, and a certain analogy in the symptoms, are we authorized to conclude that cholera has a miasmatic origin, and is but a form of marsh disease, produced by the swamps of the Ganges? The Commission does not think so. It is in fact the result of observation, that there is no proportion, on the banks of the Ganges, be-

tween the intensity of endemic miasmatic disorders and that of endemic cholera; that each has its own peculiar physiognomy, and presents its highest intensity at different times in the year. There is besides a fundamental character which separates cholera from marsh diseases, — that these last prevail at their place of origin, and that the principle which produces them is not reproduced in man, and consequently is not capable of transmission.

To sum up, *in the actual state of science, we can only frame hypotheses as to the nature of the generative principle of cholera. We know only that it is a native of certain countries of India, and that it dwells there permanently; that this principle is reproduced in man, and accompanies him in his peregrinations; that it may also be propagated to a distance, from country to country, by successive regenerations, without ever being reproduced spontaneously outside of man.*

Adopted unanimously, except by Dr. Goodeve, who refrained from voting.

XXIX.

What are the vehicles of the generative principle of cholera?

Under the name of vehicles, the Commission means only to speak of the agents by the intervention of which the morbific principle enters the organism. To this question facts answer, that the air is the principal vehicle of the cholera principle. The rapid spread of the disease in an infected locality; the simultaneousness of a great number of cases in a given assemblage of people, where mediate or immediate contact with those first taken sick was not possible; the general influence which in the time of an epidemic weighs more or less upon individuals placed within the limits of the infected district, — these circumstances, in addition to facts which show that some persons have been attacked with cholera at a short distance from the cholera focus, with which they have had no communication, — all these circumstances prove, that, in fact, the surrounding air is the principal vehicle of cholera. The principle of cholera then is volatile, and acts in this respect after the manner of miasma; that is to say, by infecting the atmosphere.

But because the air is the vehicle of the cholera principle, does

it follow that this principle can be transported to a distance by the atmosphere? On this point, facts answer in the negative. They show that the action of the cholera miasm is so much the more sure as it takes place in confined air and in close proximity to the focus of emission, whether this focus be a patient, or his dejections, or an object contaminated by them. An author of high authority (Griesinger) supposes that the probability of the action of the cholera germ, spread through the atmosphere, diminishes in the direct ratio of the square of the distance from the point of emission; that is to say, that its action will be ten thousand times more sure at the distance of a foot than at the distance of one hundred feet. It seems then that it is the same with the cholera miasm as with the miasm of typhus, that it rapidly loses its power in the open air at a short distance from its origin.

XXX.

To what distance from a focus of emission can the principle of cholera be transported by the atmosphere?

Here a question presents itself, of the highest importance in practice; to what distance from a focus of emission can the principle of cholera be transported by the atmosphere? The general rule, founded upon observation, shows that the transmission of cholera, through the medium of the open air, rarely operates beyond a very short distance (let us say a hundred metres approximatively, in order the better to express our thought); that also in an immense majority of instances, transmission has only taken place at a much shorter distance. But are there facts which would compel us to admit that it may occur much farther from the focus of emission?

The cases where cholera has been transmitted from a lazaretto to a neighboring locality would be of great weight in settling this question, if all these cases did not admit of a much more probable interpretation than that of transportation by the atmosphere. We have seen, in what we have related of the Ottoman lazarettos, that it is not the atmosphere which should be accused of transporting the disease when the town in the neighborhood of one of them is affected: it is due to passing beyond the limits, or the insufficiency of preventive measures.

The last year, at Malta, the first case of cholera occurred in the town at a distance of six hundred and sixty-two feet from the lazaretto, without its being certain that there had been any communication; but it must be added, that before this time numerous arrivals from Alexandria had been admitted to Malta without interruption. At Ancona the cholera appeared in the town after the arrival of a vessel from Alexandria which had landed a cholera patient at the lazaretto; but there are sufficient reasons for believing that it was not the wind passing over the lazaretto which conveyed the contamination to the town. It is the same with regard to Spain; the propagation of cholera in 1865 was not the result of transportation by means of the air of the disease within the lazaretto. (*For all these facts see the historic sketch.*)

The lazarettos have not furnished us any authentic case of the transportation to a certain distance of the cholera principle. It is none the less true, nevertheless, that proximity to a lazaretto is dangerous to a healthy locality.

A case of transmission to the distance of a mile was observed in Sunderland in 1848; and another in Ireland, where it seems to have taken place in the direction of a violent wind.

But the most characteristic facts are those of vessels coming from healthy countries and affected with cholera at sea, at a certain distance from an infected place, before having any communication with it. Two facts of this kind have been laid before the Commission; one is that of two vessels of the French squadron of the Black Sea, which, in sight of land and before arriving at Baltchick, had each a case of cholera on board. This fact has been already stated. It must be admitted that too many important details are wanting for this to have much value. The other is even less circumstantial; it is that of an English squadron, which, in sight of Malta, where the cholera was raging, had some cases on board. But from what place did this squadron come? and had there been an epidemic on board at that place? We are unable to say. All these cases, then, leave many doubts.

Taking into consideration all the known facts, the Commission has come to the following conclusion: *The surrounding atmosphere is the principal vehicle of the generative agent of cholera; but the*

transmission of the disease by the atmosphere, in the immense majority of cases, is limited to a very short distance from the focus of emission. As for the cases cited of transportation by the atmosphere to the distance of one or more miles, they are not sufficiently conclusive.

Adopted unanimously, except by Dr. Goodeve, who refrained from voting.

XXXI.

Besides the air, what are the other vehicles of the cholera principle?

The air is not the only vehicle of the cholera principle. The facts observed in England seem to establish beyond a doubt that *water*, whether it is rendered impure, as we have said, by matters coming from cholera dejections, or contaminated by the morbific agent diffused through the atmosphere, may be the means of introducing this agent into the organism. We need not recur to the cases which sustain this opinion.

It is also reasonable to admit, although proof in support of the opinion cannot be adduced, that certain alimentary substances may become the vehicles of the same morbific principle.

Therefore, according to the Commission, *water and certain ingesta may also serve as vehicles for the introduction into the organism of the generative principle of cholera.*

This granted, it follows, so to speak, necessarily, that *the passages by which the toxic agent penetrates the economy are principally the respiratory passages, and, very probably, also the alimentary canal. As for penetration through the skin, nothing tends to prove it.*

Adopted unanimously.

XXXII.

What are the principal receptacles of the cholera principle?

The principle of cholera, we have said, is reproduced in man by the morbid evolution to which it gives rise; but in what way does it make its exit, and what are the media, the substances, which serve as receptacles for it? Here facts answer also in a positive manner. It is in the digestive canal that the production of the morbific agent appears to be accomplished. But whether it be so or not, it is certain that the matters from the alimentary canal of

a cholera patient contain it. The observations of which we have spoken, and which prove it, are so numerous and characteristic that we are not permitted to doubt on this point. This being the case, it would appear, from the researches of Pettenkofer and Thiersch, that the morbific principle exists only in a latent condition in the fresh dejections of cholera patients, and that it is necessary for a certain degree of fermentation to take place to develop this principle, and allow it to disengage itself. This is undoubtedly only a theory, but a theory agreeing with facts, and also so fertile in practical deductions that we must give it very great weight.

The generative principle of cholera is then, finally, according to this doctrine, the result of the fermentation of the matters discharged by cholera patients; so that everything which tends to prevent the fermentation of these matters without destroying them tends also to preserve intact the germ of cholera, which may develop itself later, if circumstances favorable to the fermentation present themselves; so that also, the principle of cholera, the volatile product of a fermentation, can only have as such an ephemeral activity; and this is, in fact, what observation shows.

However it may be with regard to this doctrine, it is incontestable that the dejections of cholera patients are the first receptacle of the morbific principle, and that from them, the linen, the effects, in fine everything which may be soiled by these dejections, — and, with so much the more reason, the privies, sewers, water, the porous soil of the place, — may become secondary receptacles from which the morbific principle will disengage itself more or less quickly and more or less energetically, according to circumstances. Hence the necessity of taking into consideration all these elements, in adopting precautionary measures.

But is there not reason for believing, that the principle of cholera escapes from the organism in other ways than by the alimentary canal, by the pulmonary exhalation for example? All which we are permitted to answer to this question is, that the fact has not been demonstrated, and that, besides, the morbid phenomena which characterize cholera make it but slightly probable.

In conclusion, *the matter of cholera dejections being incontestably*

the principal receptacle of the morbific agent, it follows that everything which is contaminated by these dejections becomes also a receptacle from which the generative principle of cholera may be disengaged, under the influence of favorable conditions; it follows also that the production of the cholera germ takes place very probably in the alimentary canal, to the exclusion, perhaps, of all other parts of the system.

Adopted unanimously.

XXXIII.

What is the duration of the morbific activity of the generative principle of cholera?

This question must be regarded from two different points of view, each of which is of great practical importance.

First, for how long a time does the morbific agent, when expelled from the system, preserve its activity, or, in other words, the power of transmitting the disease? This important question is, to a very great degree, solved by facts. These show that the cholera principle has generally but an ephemeral activity; that, exposed to the open air, it is rapidly destroyed, so that, if it is not reproduced and kept up by successive regenerations, or if it is not preserved in a latent state by peculiar and very rare conditions, the locality where an epidemic has prevailed soon loses the property of reproducing the disease. This is, in fact, the result of the study of epidemics of cholera, considered under the most circumscribed conditions possible. We have seen that, in a given collection of people, the rapidity of the epidemic development is proportionate to the concentration of the mass, and that soon, if this mass is not renewed, the disease ceases there completely, until there is a new importation. In this case, the cessation of the epidemic cannot be explained, except by the combination of two circumstances: on the one hand, the complete or acquired immunity of the mass of survivors, which prevents the regeneration of the morbific principle; and, on the other, the more or less rapid extinction of this principle itself, unless it is renewed. Now, experience shows, that, as a general rule, under ordinary circumstances, a locality which has been visited by a

cholera epidemic loses the power of transmitting the disease to newly-arrived persons in a very short time after the epidemic is entirely extinct. The epidemic does not disappear because the morbid principle has lost its malignity; the proof of this is (as we have already shown), that, if in a locality where the disease is on the point of dying out, there are new arrivals of persons as yet uncontaminated by cholera influence, this will act upon them with quite as terrible effect as if the disease were just commencing; and that, if individuals go from this locality to healthy ones, the disease which they produce there has lost none of its severity. The cessation of an epidemic, then, is not because the existing morbid principle has lost its malignity, but is owing to the immunity acquired by the population where it has been raging, combined with the rapid extinction of the principle itself.

Such is the general rule, we say, but we are compelled to admit that there are important exceptions. Certain facts prove, that, even in our countries, certain localities have been able to preserve the cholera for many years in succession, without this continuance of the disease being entirely explained by the renewal of the populaion. It seems that here some peculiar conditions, either in the soil, or in the usages of the place, have contributed to prevent the rapid decomposition of the morbific principle; whence arise the sort of efflorescences which have been observed at certain points in Europe, after great epidemics. It should be remarked here, that the study of the circumstances under which these exceptions are produced may put us in the way of discovering the causes of endemic cholera.

But the exceptional facts which are most interesting from our point of view are those which show that the linen, the effects of a cholera patient, may retain for a long time, when fresh air has been excluded, the property of transmitting cholera. These facts prove, that, under certain conditions of confinement, the cholera principle may retain for many months perhaps, in a latent condition, a sort of vitality which will show itself on contact with the air. But, if the fact is incontestable, examples which prove it are too rare to permit us to draw from them any precise law with regard to the time during which, in a state of confinement, this morbific activity may last.

Therefore, according to the Commission, *it results from the study of facts, that in the open air the generative principle of cholera loses rapidly its morbific activity, and that such is the rule; but that, under certain peculiar conditions of confinement, this activity may be preserved for an undetermined period.*

Adopted unanimously.

There remains yet the question, for how long a time the morbific principle may be reproduced and eliminated by the organism, or, in other words, for how long a time an individual affected with the premonitory diarrhœa or confirmed cholera, can transmit the disease? This question, upon which depends the duration of quarantine, is very difficult to solve, and it has been very much discussed in the Commission. It was particularly with regard to the possible duration of the diarrhœa called premonitory, which, according to experience, has, like confirmed cholera, the property of transmitting the disease, that the discussion was carried on.

The opinion that this infectious diarrhœa may in certain cases be prolonged for many weeks has been sustained with much force, having in its support considerable authority, and especially that of Griesinger. Cases have been cited of individuals, affected with diarrhœa for many weeks, who transmitted cholera, and finished by succumbing themselves to the disease; now, as during an epidemic it is impossible to distinguish, by peculiar characters, cases of this kind from those which cholera has nothing to do with, it follows, we conclude, that all these cases of prolonged diarrhœa should be considered as suspected.

To this it has been replied, that it is matter of observation that the diarrhœa called premonitory generally lasts not more than three days, and that, if it is prolonged beyond that period, it very rarely lasts more than a week; that nothing shows that the cases on which the opinion is founded were not cases of diarrhœa of an entirely different nature from cholera, in the course of which this has come in, as is very common during an epidemic; that it is with the disease as it is with the period of incubation; that the immense majority of cases prove that both are very short in cholera; and that consequently we may in entire security regard as uncontaminated an individual isolated from all cause of infection, in whom the diarrhœa is prolonged more than eight days after his

isolation, without his having presented any characteristic sign of the disease.

Finally the Commission has adopted the following formula:

Observation shows that the duration of the choleraic diarrhœa, called premonitory, — which must not be confounded with all the diarrhœas which exist during the time of cholera, — does not extend beyond a few days.

The facts cited as exceptional do not prove that the cases of diarrhœa prolonged beyond that period belong to cholera, and are capable of transmitting the disease, when the individual affected has been kept from all cause of contamination.

Adopted by fourteen votes against four. Messrs. Gomez, Millingen, Mühlig and Salvatori voted in the negative; M. Monlau refrained from voting.

Here end the labors of the Commission with regard to the origin, the endemicity, the transmissibility and the propagation of cholera, and the historic sketch of the course of the epidemic of 1865, made by a sub-committee, of which Dr. Bartoletti was the Secretary, before being presented separately to the Conference.

In answering, as they have done, the different questions placed upon the programme, — that is to say, by limiting themselves to drawing from facts the consequences which reasonably flow from them, — the Commission thinks it has established sure foundations which will enable the Conference to pronounce understandingly upon the questions relating to prophylaxia.

A. FAUVEL, *General Secretary.*

The present report, having been discussed and adopted, chapter by chapter, was approved as a whole by all the members of the Commission.

CONSTANTINOPLE, May 21, 1866.

MEMBERS OF THE COMMISSION.

Bartoletti, A. Bykow, F. Bosi, E. D. Dickson, A. Fauvel, E. Goodeve, B. A. Gomèz, Baron de Hübsch, Comte A. de Lallemand, E. Lenz, A. Maccas, J. Millingen, P. F. Monlau, Mühlig, Comte de Noidans, E. Pélikan, J. E. Polak, Salem Bey, S. Salvatori, Sawas, A. M. Segovia, A. Sotto, I. Spadaro, J. Van-Geuns.

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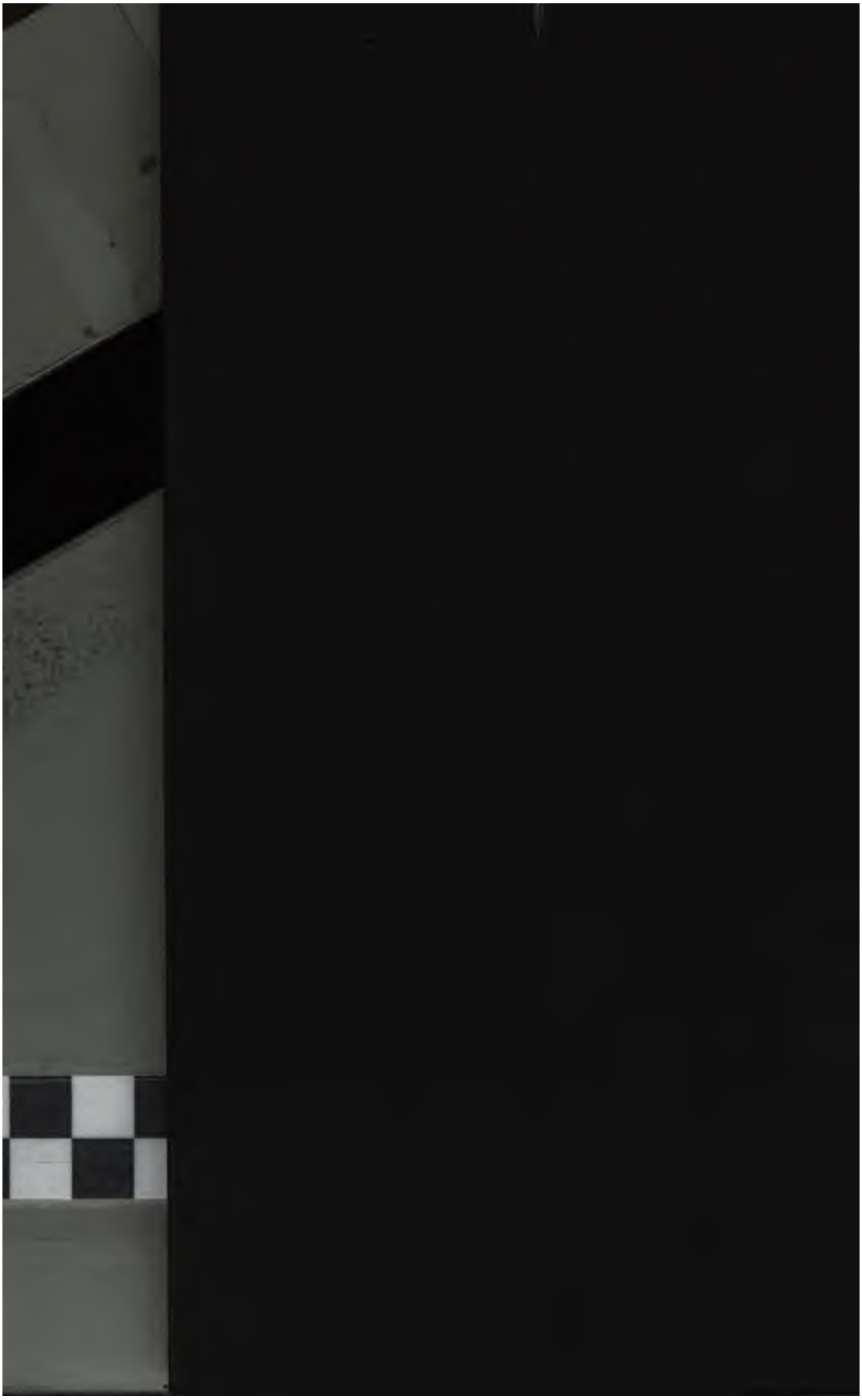
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